ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY

ARISING FROM THE USE OF ASBESTOS IN ONTARIO

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180 Dundas Street
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Friday,
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Volume XXIX

ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY

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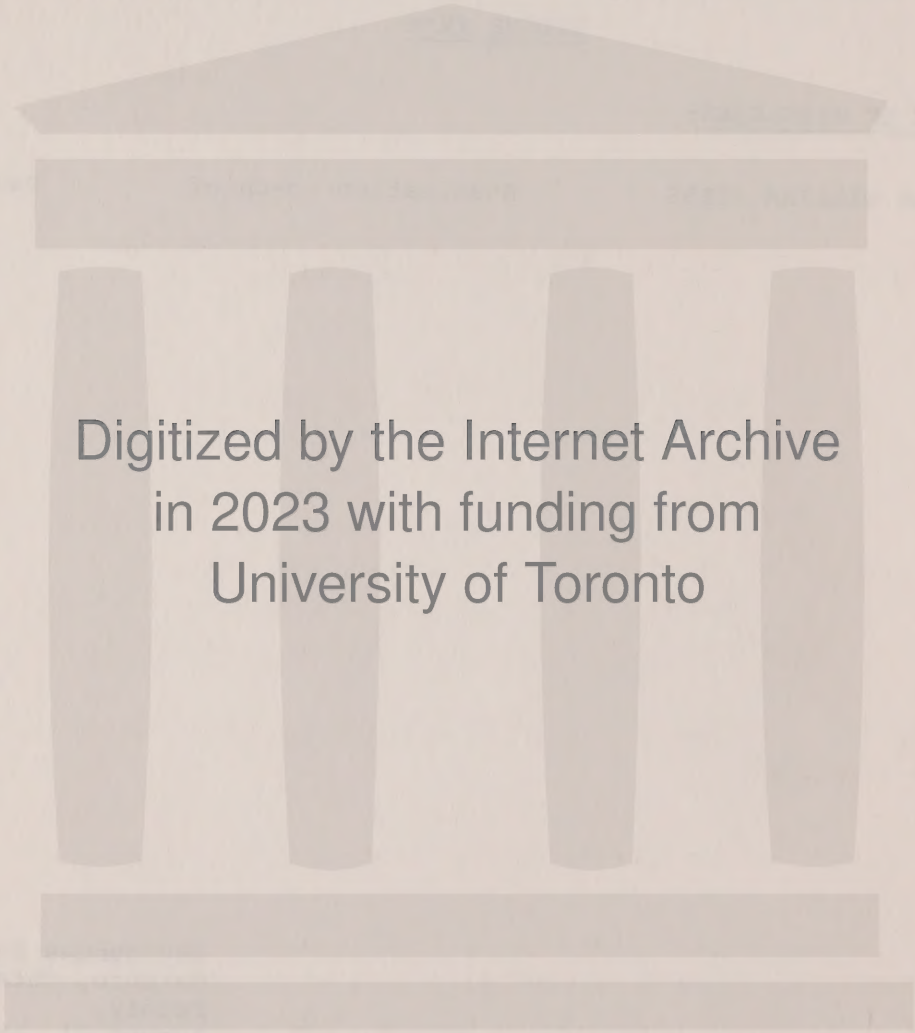

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THE FURTHER PROCEEDINGS OF THIS INQUIRY RESUMED PURSUANT TO ADJOURNMENT

APPEARANCES AS HERETOFORE NOTED

DR. DUPRE: May we convene, please, ladies and gentlemen? M. Casgrain, may we convene, please?

Dr. Mustard is unavoidably detained, but will be here within the hour. I suggest that we proceed at this time.

Counsel, have you any announcements before I greet the witness?

MR. LASKIN: I don't believe so, Mr. Chairman.

DR. DUPRE: Do the parties have any items to raise?

Well, may I then, on behalf of all of us, greet Dr. Graham Gibbs with double warmth. Double warmth because, of course, we are already much in his debt since he was the victim of our scheduling last July which forced us to cancel his initially-scheduled appearance, and I therefore, Dr. Gibbs, do indeed greet you all the more warmly, sir. Your reputation precedes you. Your name has been used in vain by a number of expert

DR. DUPRE: (cont'd.) witnesses who appeared before us, and we very much look forward to the instruction you will provide today.

Miss Kahn, will you swear in the witness, please?

GRAHAM WILLIAM GIBBS, SWORN

MR. LASKIN: Mr. Chairman, just before I start with Dr. Gibbs, I should formally introduce Mr. Paul Hess, who is the senior counsel at the Ministry of Labour, and he is here on behalf of the Government of Ontario this morning.

MR. McNAMEE: He's standing in for me this morning, as a surrogate.

MR. LASKIN: In the back row beside Miss Jolley.

EXAMINATION-IN-CHIEF BY MR. LASKIN

Q. Dr. Gibbs, your...in front of you are two volumes containing several of your publications, and just for ease of reference you will see that they are tabbed, and if we refer to them we will refer to them by their exhibit number, which is forty-one, and the tab number in there.

Your curriculum vitae is at tab sixteen, in fact, and just very briefly, I gather that your training is both in geology, chemistry, geological sciences on the one side and epidemiology and medical statistics on the other?

A. That's correct.

Q. You were, until very recently, at McGill University and were both a professor and director of the Occupational Health and Safety unit?

A. That's correct. I would add that I maintained a position, part-time position with the University since I left last September.

Q. The position you have just left at McGill, as I understand it, is the position that Dr. Corbett McDonald is

Q. (cont'd.) succeeding to?

A. That's correct.

Q. What is your present position with the Celanese Corporation?

A. I am director, health and safety affairs.

Q. For the corporation?

A. For the corporation.

MR. LASKIN: Mr. Chairman, Dr. Gibbs is going to be good enough to address a number of the issues which are of concern to this Commission, in part with the use of slides and in part with the use of the overhead projector.

Just to forestall or predict one matter, I haven't seen either the slides or the overhead projector reproductions, but I gather from Dr. Gibbs that there will at least be a number of them that are not found in any of the articles.

We will do our best to provide them to the parties, and indeed to yourselves, afterwards. Let's just hope that we can simply identify them by what they show as we go along.

If that's convenient to all the parties?

DR. DUPRE: Agreeable?

UNIDENTIFIED SPEAKER: Yes, sir.

DR. DUPRE: Proceed then, please.

MR. LASKIN: Dr. Gibbs, we are in your hands.

DR. DUPRE: You may wish to give Dr. Gibbs the usual warning about our recording microphones.

MR. LASKIN: Yes. Good idea, Mr. Chairman. What you have in front of you, Dr. Gibbs, is not a microphone, but a recorder for the benefit of our court reporter.

As best you can, if you can keep close to that instrument it will help everybody and preserve the record we are hoping to keep.

THE WITNESS: Fine.

THE WITNESS: (cont'd.) I think before starting what I should do is inform the board and members here that I left the university last September, and although I have continued with a number of studies on asbestos since that time, I have not been as deeply involved in asbestos as I was during my time at the university.

Although I have remained as a member of certain committees evaluating research projects, and have kept abreast, to a certain extent, of the literature in the last few months, as one must recognize when you drop out of a field as your full-time, almost full-time occupation, you perhaps get a little bit less familiar with the current literature than you do without.

So I would like to mention that before we start.

What I thought would be an appropriate way to address the question of asbestos and health hazards associated with asbestos would be to lead you through some of the research activities that I have been involved in over the last twenty years or so, and I have done this - preparation has taken into account the questions which Mr. Laskin provided me with which he thought were of particular importance to the board - some of the issues which had been raised, and so I will attempt to answer some of those during the course of my presentation.

I think that people should feel free, perhaps, to interrupt me at a particular point if there is something that is not clear, rather than try to recap, to find an appropriate slide at some point later - if that's acceptable to you, Mr. Chairman.

I first got involved in the problem of asbestos around 1959, 1960, when I was working with the British Medical Research Council. This was about the time that Wagner, Sleggs and others in South Africa first reported the occurrence of mesothelioma in people living in the area of the Cape crocidolite

THE WITNESS: (cont'd.) asbestos mining area in South Africa.

One of the first issues which came up resulted from some work being done by Jack Harington in South Africa, on the extraction of oils from asbestos.

I am going to address that a little bit in terms of some of the work that I've done, later. I began some work while I was the MRC on this question, because it was thought at that time that maybe the oils - and there were some polycyclic aromatic hydrocarbons associated with those oils - might be the explanation for the carcinogenicity of the crocidolite fiber. That was back in 1960.

Now, I'm going to jump ahead a bit to 1966, when I first joined McGill University. I came to Canada to the Department of Epidemiology and Health, and the first assignment on which I worked was one on asbestos. This was part of the series of studies Dr. Corbett McDonald would have described to you at a previous hearing.

One of the first things in studying the health effects of an occupational group is to understand exactly what these people are exposed to, and maybe if I could have the first slide...it's possible they will show without the light.

DR. UFFEN: May I just ask a question while we are waiting?

THE WITNESS: Sure.

DR. UFFEN: Is it possible that for you very briefly - I don't want a big long explanation - to explain how oils became associated with asbestos?

THE WITNESS: Yes, I'll deal with that with some data, very briefly, later.

DR. UFFEN: All right.

THE WITNESS: First of all, I'm sure you are all familiar with such a table. The first thing we have to recognize is that we are dealing not, under the term asbestos with a variety of minerals.

These minerals are quite different from one another. If we look at the table on the slide, we can see that under the chemistry, if you like, of chrysotile, it is quite different from the chemistry of the other fibers. We see that within the amphiboles, the chemistry of the different amphiboles is different.

In practice, when we are dealing with minerals we are not dealing with a straight chemical. In other words, if we talk about sodium chloride, we know it's NACL. When we are talking about the asbestos minerals, we have the opportunity for substitutions and for example, tremolite and actinolite, although they are not shown as such on there, are really part of the same series of minerals, and we can range all the way from the very high iron at the one end, to a very calcium at the other end, and we end up with ferroactinolite through tremolite.

We have every combination, almost every combination of chemical substitution in that range. So when we have an asbestos fiber of tremolite, it probably is not purely of that chemistry.

Now, I think it's important to realize that when we are dealing with minerals we are dealing with very complex systems, and we use these general terms to define certain ranges of composition.

I don't intend to go into any more detail in terms of the minerologic characteristics of asbestos, but to start with we have to realize we are dealing with quite a complex system.

Can I have the next slide, please?

Not only do these materials differ in chemistry, they also differ very much in structure. This gives you some

THE WITNESS: (cont'd.) idea of the structure of chrysotile, plinochrysotile, and gives you some indication of why this curling or curvature of the fiber - because of the different sizes of the cation in the structure.

Can I have the next slide, please?

On the other hand...that's fine, you can leave it, it doesn't matter which way one looks at it...the point is to make that here we have quite a different structure associated with the amphibole. So structurally there are differences between chrysotile and the amphiboles. Within the amphiboles they will all have a similar structure to this, but the atoms in the structure will change, and the chemistry will change, and certain physical and chemical characteristics of the fiber will also change.

MR. LASKIN: Q. Can you just elaborate on these structural differences? I mean, we are seeing something different between that slide and the slide before, but can you point out to us what...?

THE WITNESS: A. Without getting into any detail... if we can go back to the first slide, please. In the first slide you will see the chemistry of chrysotile, and you will see the chemistry, if you like, of the amphiboles.

If you look at the amphiboles, you will see that they all have an Si_8O_{22} - OH twice - component. If you want to multiply up...you see they are shown on here as $_8Si\text{-}O_2$ on the structure. If you count up all the oxygens in actinolite or tremolite or anthophyllite or amosite or crocidolite, you will find you have eight silicans - Si_8 - you will find the oxygens will total twenty-two, and you will find there is an OH twice at the end.

In the amphibole structure, the chains that are drawn up accomodate the Si_8O_{22} component, and the other atoms have to be fitted in to that structure.

THE WITNESS: (cont'd.) In the chrysotile situation you have, in essence, a situation where you have a magnesium hydroxide component - the magnesium and the water component go to form a magnesium hydroxide component, and the sizes of the various atoms that are in there are such that these material cannot form a straight structure. The sizes of these are such that you begin to get curvature in order to accomodate the different atoms that are in the structure.

So what you have are two distinctly different structures for chrysotile and for the amphiboles. The amphiboles are all very similar, but the chemicals of the atoms substituted or appearing in the lattice are different from mineral to mineral, and this is why the two are so distinct.

I don't know whether that clarifies, without going into detailed crystallography of the materials.

DR. UFFEN: Remember Mr. Trudeau was explaining? It's the same, but different words.

THE WITNESS: Okay. Fine.

Now, one of the...maybe I would like to go to the overhead now.

Before I leave the question of the differences in chemistry, we have to also recognize it is possible with chrysotile to have some substitution, and there is a mineral called garnierite where in fact nickel has substituted in the structure. It's a very rare mineral and it's not going to be a problem, a concern, to us, but I would just like to demonstrate that we are not dealing with a specific chemical.

Now, when I started work in the asbestos mining area in terms of to study the health effects, with Corbett McDonald's team, one of the things we want to know is what are workers exposed to. First of all, in the slide you can see that we have to consider asbestos itself, certain naturally-occurring

THE WITNESS: (cont'd.) contaminants such as host rock, accessory minerals, alteration products, minerals derived from intrusion of neighboring ore bodies. We had to consider the possibility of organic compounds because of the reports by Harington in South Africa. We had to consider the possibility of variations in crystallinity and mineralogy, variations in chemical composition and properties, variation in physical properties of the fiber, and as a result of processing whether other contaminants might be added - trade metals, organic contaminants and so on.

Now, the reason for this perhaps is not immediately evidenced, why one needs to know all the detail about what we are working with. But we have to recognize that we really, at the time that we started the studies, it had been demonstrated that there was an increased risk of lung cancer in relation to asbestos exposure. It had been demonstrated that there was an increased rate of mesothelioma in certain occupational groups. But as far as etiological factors, what was responsible for these effects really was not known, and there were a number of hypotheses around that maybe any increase in risk was related to trace element - nickel, chromium and cobalt; that it might be related to polycyclic aromatic hydrocarbons; that they might be related to other contaminants of the rock.

Secondly, we really needed to know whether our workers were exposed to materials which themselves might be well recognized as producing carcinomas, so if we didn't gather this information we would be left with a gap in knowledge.

Now, to give you some idea of the potential complexity of studying a mining population, we might go back to the 1964 New York conference.

The 1964 New York conference, National Academy

THE WITNESS: (cont'd.) of Sciences, it was decided we needed information on pure chrysotile exposure, and the studies at McGill were undertaken in order to study a group of workers who were exposed only to chrysotile fiber.

In large part, of course, that's true - these workers are exposed to predominantly chrysotile asbestos fiber, but there are a number of other minerals that do occur in minor content in the mine, and perhaps later in the presentation we can discuss some of the ways in which some of these other minerals might be important.

This is just a list of minerals which I compiled by going through some of the publications, and discussions with the geologists of the various mines in the Eastern Townships back in 1970.

Is it clear for..and you read it?

MR. LASKIN: It comes from - the last two overheads are from tab three of the exhibit, pages 785 and 786.

THE WITNESS: Can I have the next...yes. Leave that one in, yes, please.

It's also important to realize that some of the minerals which are in that list can themselves be fibrous, and therefore if we evaluate the airborne concentration of fibers in the mines we have the possibility that we are not always measuring chrysotile asbestos.

Now, the components, the composition of the airborne fiber, is not going to contain a very high proportion of these other fibrous minerals, generally. Nevertheless, they can exist, and this is an example of nemolite fiber. It's a fibrous brucite which occurs in the mining industry, and we may also find some tremolite in small amounts in different places, which is fibrous. We can also find some anthophyllite, and so on.

So we have small amounts of other fibrous materials

THE WITNESS: (cont'd.) in addition to other known fibrous materials that would occur in the mines.

Could I have the next slide, please?

One of the first things...sorry...one of the early things that I did in Canada was to look at see whether or not there was any evidence of any organic material in chrysotile fiber, and to do this I collected what were in essence virgin samples of Canadian chrysotile fiber, and we had to go out specially to find these samples so that they were free of any blasting oils or any other contamination from normal processing.

Then we extracted these samples with organic solvents to determine the residues of extractible material, and you can see that very small quantities of organic material was found in the Canadian chrysotile fiber.

As far as the origin of some of these, which was your question earlier, what I believe might have happened is that some of the surrounding ore bodies, some of the surrounding country rocks, include some graphitic schists, include some shales, and there were intrusions into those shales, and it's possible that some of the organic material got cooked off at some point and absorbed into the fiber, or during the metamorphic phase some of these organics got carried in. It's a very low quantity of material.

There is a thesis on this subject and the source, I think, is likely to be that. It's very, very difficult, of course, to find pure, virgin samples any more. Even some of the outcrops we went to, which the geologists assured me were... absolutely nobody had ever touched these locations and we could get right in, we would get there to find that somebody had been in a little while before on some explorations and had blasted the whole area.

So we had some difficulties in finding the samples, but it appears there are small amounts of organic

THE WITNESS: (cont'd.) material in the fiber - in the raw fiber.

Can I have the next slide, please?

MR. LASKIN: Let's just, while we are going I suppose we might identify that slide. It comes from tab twenty-two, page 522, table two.

THE WITNESS: Having recognized there might be some organics in the raw material, we then looked to see whether or not there might be any addition of organics during the milling process, and this was a study where we took commercial fiber entering the dryer and leaving the dryer, to see whether there was an increase in the amount of organics and whether there were any differences between coal-fired drying - because coal had been used in the past and was still in existence at one mill - and oil, and you can see in fact some variation in here, but no consistent pattern that a lot of organics is added during the drying phase. In fact, some seem to have decreased a little bit.

In the...but the levels here are somewhat higher than we found in the raw materials, and it appears that there is some contamination by organic materials of fiber as it goes through the milling process.

Now this is perhaps fairly easy to seek out where sources might be during mining now, fuel oils and ammonium nitrate are used in blasting in the mines. We have trucks driving around which need lubrication. In the mill, all the equipment has to be lubricated and so on. So the opportunity for contamination by organics within the process is quite high.

Could we have the next slide, please?

MR. LASKIN: That last slide, for the record, was table three from the same previous reference.

THE WITNESS: Now, in terms of the components in the naturally-occurring fiber, it appeared these were mainly normal alkanes, and this is just a GC spectra and this also appears in one of the papers that you will have. Do you want me to find that, or...?

So there were normal alkanes or paraffins in the oil.

Can I have the next slide, please?

Now, what had been a possible concern was whether or not there were hydrocarbons, polycyclic aromatic hydrocarbons present in the fiber, and we looked for, but did not detect, benzo(a)pyrene, anthracene or the benzœanthracene series...oh, a small amount of the benzoanthracene at the bottom...but in terms of the concentrations of polcyclics in the fiber extracted from the commercially-milled Canadian chrysotile, the amounts were extremely low, and in subsequent work we looked at the airborne concentrations of the polycyclic aromatic hydrocarbons in the mills and we found that they were very, very low in comparison with urban air in general.

So what we had was a demonstration that yes, workers could be exposed to some polycyclic aromatics, plus some normal alkanes associated with the fiber, but that their levels of exposure were considerably less than would exist in any city, to these materials.

It was unlikely, unless there was a major synergism between these materials, that this was a factor in the carcinogenic activity of chrysotile fiber.

Can I have the next slide, please?

One of the side effects that came out of this study was of interest and perhaps might still be of interest in terms of demonstrating the surface activity of certain types of asbestos.

THE WITNESS: (cont'd.) In the mid-1960's, Dr. Timbrell from Cardiff initiated, under the UICC, the preparation of standard samples of asbestos for studies, as reference materials in biological studies and chemical studies, and what he did, he collected these samples together in South Africa, and for circulation put them in small polyethylene bags.

Now, we followed their advice and approach, and when we started studying organics in Quebec mines, we also collected our samples in polyethylene bags.

One of the things we noted though, was that the samples, most of the samples were producing an ultraviolet spectrum which were very, very similar, and some of the solutions we had became quite yellow. We got a little bit worried about that, so we collected another series of samples in glass containers, did analyses and there were absolutely no peaks in that region and no color.

What had happened was that one of the antioxydants in the polyethylene, butylated hydroxitoluene, had undergone an oxidation to form a compound, 3,3', 5,5' -tetratertiary butyl diphenoquinone. This material is yellow, it had a spectrum which was identical to what we were finding, and it was important because at the same time when both Dr. Commins in the U.K., and ourselves in Canada, were looking at UICC samples, we were finding some of them contained a very high content of organic material, and we were also finding they also contained this.

As a result of that work, the UICC samples in polythene bags were changed. All new shipments went in metal containers inside...with another internal surface.

Now, the reason this is important is perhaps twofold. One was, we had the possibility of doing animal experiments with materials which might be contaminated with

THE WITNESS: (cont'd.) something which we didn't know what it would do to them.

Secondly, we had the demonstration of what seems to be a fairly good catalysis, under certain circumstances, for alterations to adsorbed materials.

This area of the possible importance of adsorbed materials has been looked at a little bit in that Wagner in Cardiff took some UICC samples and had them extracted with benzene, and then repeated a series of studies for mesothelioma production, with the extracted materials. He found he got the same result whether they were treated or not.

So in terms of mesothelioma, it seems that the adsorbed material wasn't a factor at all. We had not seen any data in terms of fibrosis or in terms of lung cancer, but one might perhaps guess that the effect is likely to be small, but couldn't be totally ruled out.

Some surface coating with organics might affect the behaviour in tissues.

Can I have the next?

I mentioned earlier that around the mid-1960's there was a tendency to believe that perhaps some of the carcinogenic action of fibers was in fact due to adsorbed trace materials. Dr. Crawley in the United States had done a fair amount of work in trace metals in textile industries to see...and found fair amounts of trace metals. He also initiated some work which came out somewhat later on the interference between metals and some of the alloys, if you like, which occur in association with the fiber, demonstrating that combinations of metals were important in terms of the production of a tumor in an animal.

Now, this of course doesn't simplify things, in that you may have, when we are looking at carcinogenicity of asbestos, maybe many more than one single factor operating in terms of production of a tumor, and unfortunately Crawley

THE WITNESS: (cont'd.) pursued his work in relation to asbestos, which I think was a bad mistake, in that he had some rather interesting data on how metals interact in producing carcinogenesis, and the asbestos situation was far too complex a system to really get true answers.

Nevertheless, when we looked at the fiber concentrations in fiber, we could see quite large differences between different means. You can see, if you were looking at the mortality from lung cancer, for example, in the mines E and F, and we had found them to be quite different from those in that other group of mines, one might be asking the question 'does nickel play a role'.

What we were trying to do was to gather together the information which will permit us, or help us, in interpretation of any differences between mines, at a later date.

Could I have the next slide, please?

DR. UFFEN: Is that one in any of our...?

THE WITNESS: Yes, that one is in...I don't know whether the diagram per se...the data re in the qualitative aspects of asbestos.

MR. LASKIN: Tab three, I think.

THE WITNESS: Number fourteen...it's marked number fourteen on this index.

MR. LASKIN: I believe it's tab three of our record.

Linda, maybe you could show the witness.

THE WITNESS: Yes. Yes, that's correct.

MR. LASKIN: But not the...

THE WITNESS: The diagram per se is not there, but the trace metal concentration in the commercial fiber are shown in table four, the nickel content of commercially-milled Canadian chrysotile - page 790.

MR. LASKIN: Q. Is that what we are looking at on the slide?

THE WITNESS: A. Yes. You are looking at a plot, if you like, of the concentrations that are given in that table, which show that mine E and F, which run in the group seven fiber up to two thousand, are much higher than in the other fibers.

DR. UFFEN: Would that be in that mineral you called thornerite, I believe?

THE WITNESS: Garnierite.

DR. UFFEN: Garnierite.

THE WITNESS: No. I've not seen any evidence of garnierite occurring in the mines. Where some of the nickel does occur is in a mineral called - it's an alloy called awarelite, which is a nickel/iron alloy.

DR. UFFEN: Is it fibrous?

THE WITNESS: No, that's not fibrous. Not to my knowledge.

DR. UFFEN: The things that are running around in the back of my mind at the moment are the relationship to other nickel deposits in Canada, and whether or not they are fibrous and so on. So in your dissertation if you see any implications of this...

THE WITNESS: I'll mention...

DR. UFFEN: That's what's on my mind.

THE WITNESS: Yes.

The awarelite, as far as I know is not in fibrous form. The difficulty we have, of course, is that many, many minerals can occur in a fibrous form under the right situation, so I say to the best of my knowledge awarelite is not fibrous. But that's where a lot of the naturally-occurring nickel occurs.

Some of the nickel in the commercial fiber might also get in because of abrasion of equipment during milling.

THE WITNESS: (cont'd.) In fact, in the South African mills of crocidolite, manganese gets added to the fiber to fairly large extent in that way. Because it's a very hard rock, they use manganese steel balls for the milling procedure, and those manganese steel balls don't last very long in breaking up the hard rock, and that metal has to go somewhere so it goes on with the fiber through the process, and some ends up in the final fiber.

There have been some studies carried out on that fact, the addition of metals to fiber during processing.

THE WITNESS: Q. Dr. Gibbs, just before you leave that slide could you explain...what is the calculation on the vertical, or the Y axis?

THE WITNESS: A. The number of micrograms of nickel per gram of fiber. So we have taken...

Q. It's a mass...

A. It's a mass of nickel per mass of fiber, yes.

Q. What was...I'm sorry...what was on the bottom?

A. On the bottom is the grade, the commercial grade of fiber, group of fiber, and I think it will be more appropriate for one of the industrial people to give you specifics on what group three, group four and so on mean. It's a grading system where the longer fiber tend to be the lower numbers. Three and four would be longer fiber than six and seven.

This was to look, again, to see whether trace metals in their own right could be of importance in the mills, and you can see that the maximum concentration at three mills studied, for airborne nickel, chromium, manganese and cobalt, were all below the threshold limit values in existence in 1969 in those mines.

Nevertheless, there was some chromium, and there was some nickel present in the airborne dust. Nickel and

THE WITNESS: (cont'd.) chromium, of course, were of particular interest because nickel in itself had been questioned as a carcinogen, and chromium, hexavalent chromium also. So those two were included in looking at the material particularly because of that question.

So it appeared to us that trace metals alone were unlikely to be the reason for any observed carcinogenic action of chrysotile we might find later. The possibility they played some role in carcinogenesis, of course, can never be totally ruled out, but it seems unlikely they were going to explain the carcinogenic action of chrysotile.

In addition to looking at trace metals and organic materials, what minerals might occur in the mine? We also looked at the question of radon daughters. Again, because we had some evidence that radon daughters could occur in mines which were not primarily uranium-producing or radium-producing mines, such as the Newfoundland fluorspar mining areas, and the results of those measurements in the mines were that the concentrations of radon were all extremely low, and that is reported in the same paper we just referred to.

So it appeared that we were not going to be able to hang any health effects of chrysotile on radon daughters.

Yes, if you will leave that in, please.

Now, before I move now...sorry, we'll go back to the...before moving into the area of dust assessment, we've so far looked at qualitative aspects - what were people exposed to.

Now I would like to move into some of the quantitative aspects of dust exposure in the mining industry, and the first thing perhaps to understand a little bit is what happens in a mining industry.

First of all, we have primary drilling. The rock in the mine is drilled, then a charge is put in, the rock is blasted,

THE WITNESS: (cont'd.) large chunks of rock then are broken up again by further drilling and blasting, rock is shovelled into trucks, transported to a jaw crusher. From the jaw crusher the rock might go to rock storage and some will, depending on circumstances, perhaps most of it will go through a vertical dryer from the crusher directly, and then into rock storage. From rock storage into hammer mills and fiberizers, screened where the fiber is removed by aspiration. The rock which passes on goes into further treatment, and eventually we end up with fiber which has been aspirated, when then goes into the cyclones, into a grading system, into a fiber bin, then is bagged and shipped.

Now, when we start...can I have the next slide, please...it's important to bear in mind that these operations take place in the pit, sometimes in very large areas, in the mill, sometimes in buildings which have many, many floors, and the areas in which people work in those buildings are quite large.

Historically, perhaps to give you some idea of how things operated in the mining industry, when we put together our assessments of fiber exposure, we looked through discussion with people in the industry, through review of existing documentation on the mining industry - what were the processes in the mining industry and how did people function?

When, for example...how was rock loaded in the pits, back at the turn of the century? What was hand-cobbing? Some of the early fiber would have been removed, if you like, from the fiber by knocking the excess rock off the side of the fiber.

As we mechanized more, the earlier techniques began to drop out and we found the more efficient ways of both extracting the fiber and of removing the fiber from the mine.

THE WITNESS: (cont'd.) But in essence, when you get down to it, the procedure to produce chrysotile asbestos has been very much the same for many, many years - the actual milling procedure has not changed too much in many years.

As far as controls in the industry were concerned, some attempt was made in some companies back in the 1938 area to try and put some controls in. The majority of environmental controls, though, took place perhaps during the late 1940's, early 1950 period, and later on when I show you some of the dust concentrations in the mills you will see the sort of effect of the introduction of these environmental controls at that point in time.

But the methods of handling the fiber in the mills, where the people came in direct contact with it, were quite different from what they are today.

Now, I wasn't able to observe how this was, how fiber was actually put into bags many years ago, but through discussions with past employees, from putting together the information on the mining industry, certainly the opportunities for exposure were considerably different from what we are talking about in the present day mill.

Let's have a look at measurements within the industry. Why are measurements made?

First, they are made to identify sources of fiber. They are made to check control measures, they are made to check compliance with standards. They may be used to assess the effectiveness of controls, and sometimes they may be to determine the magnitude of exposure.

One of the things we have to realize in considering the data that exist in the Quebec chrysotile mining industry and in virtually every other study in which we are assessing the health effects, is that the raw data on the

THE WITNESS: (cont'd.) environment were not collected for epidemiological studies. They were collected for other purposes. They were collected for control purposes to ensure that the mines were in compliance with some recommended levels, even if there was no legal standard, and also to see where control was needed.

Can I have the next slide, please?

MR. LASKIN: Q. You may be coming to it, but are you suggesting some implication from that in terms of the accuracy, or otherwise, of the measurements that were made?

THE WITNESS: A. Well, I think...no. What I am, the point I would like to make is that if one were designing an epidemiological study today, a prospective study, one would make every attempt to design your methods of sampling to respond to the questions you are asking of the data.

What one is doing in all cohort studies, or retrospective cohort studies, one is working with existing data, and when you are working with existing data you are not there in order to collect the data, you are not there to ensure the quality, you are not there to determine why exactly a particular sample was taken.

Now, I will address this a little bit later, because in our particular case in the Quebec asbestos mining industry we were very lucky in that regard, because Maurice LaChance, who was the engineer for the Quebec asbestos mining industry, had been with them since about 1949, and had been responsible for making all those measurements up to 1966. So we had the opportunity to work with that engineer on going back through his data and determining what his notations in his working books were in terms of whether this was an unusual situation or a usual situation, and so on.

Very rarely do you have that opportunity, to be able to

THE WITNESS: (cont'd.) complete data where you have the person who actually made the measurements, that he would be there to tell us, oh, this was essentially for control purposes, we were looking to see whether there was a leak here, and so on. So we were able to look at his data in that light. Very rarely do we have that opportunity.

So then we have to recognize that usually we are dealing with data which are not collected for the purpose we would like to use them, and we have to make the best we can of them.

This was just to show how chrysotile production has increased in Canada. I'm going to...can I have the next one, please?

That is in a publication already. This is to show also how amosite, Cape crocidolite, Transvaal chrysotile, also have increased. That is in the...

MR. LASKIN: It's in tab thirty-two, I think, your surveillance paper.

THE WITNESS: That's right, yes.

To show that production has increased quite drastically since the 1920's in South Africa, and since 1878 in Quebec.

Could I have the next slide?

In this slide you will see the dust concentrations... this is in the paper by Gibbs and LaChance...up until 1966. These figures have been extended beyond the period in the original paper by Gibbs and LaChance.

You can see that the concentration in million particles per cubic foot, these are the concentrations measured by the midget impinger technique, which I can describe in more detail if anybody so requires... made in the industry since about 1949.

THE WITNESS: (cont'd.) One of the things that you will see, is that there has been a steady drop in concentration since the beginning of the 1950's, and from review of the... with workers and management at the mines, we understood that the...and from Maurice LaChance's own personal experience...the conditions in 1950 and 1949, at most mines, were very similar to those that would have existed in the milling part of the industry prior to that time, because there was very little in the way of environmental control in most mills.

The method of dust collection, and so on, was relatively crude...if it existed at all.

So the environmental conditions in 1950, around that period, gave us a pretty good idea of what things were like prior to that period.

Can I have the...yes, sorry?

MR. LASKIN: Q. Before you leave that, it's reproduced at page 171 of the article.

When you refer to the worst mill, the average mill or the best mill, is that a characterization taken on a year-by-year basis? You are not following one mill through time?

THE WITNESS: A. Originally we plotted this information for each individual mill, and there were two mills which were distinctly different. One mill, where they had started to make some environmental control back in the 1938 or thereabouts, the mid-1930's, that particular mill had kept ahead very much of the other mills in terms of what it had done to reduce environmental conditions. So we found consistently lower concentrations, if you like, in that mill, and we considered that the best mill, if you like.

The other mill, which we considered to be the worst mill, was just that. Conditions had been just a bit worse than the others.

THE WITNESS: (cont'd.) But when you look at the mean there, the average, of course, there were other mills which were...not as bad, but very similar to the top one, and others which were better in their environmental conditions and closer to the bottom line.

Q. Is the best mill that you've got there one mill that you've followed through...?

A. It's one mill, yes. It's one mill.

Q. ...time from before 1950 all the way through to 1975?

A. That's right. That's right.

DR. UFFEN: Could I ask a point about the oscillations?

THE WITNESS: Yes.

DR. UFFEN: It may be sort of accidental. It seems to have a period of about three years, but could we assume that if you projected back beyond 1949, that similar oscillations for the worst mill might have existed?

THE WITNESS: I think it's difficult to interpret what the oscillations mean. I think that one of the problems that I would discuss in a moment, in relation to the environmental data base in the industry, is that during this period, 1950 through to 1966, one person in the industry made all the measurements in all the mills.

Now, that's not totally true in that one or two companies had some additional measurements made by themselves, and there were some insurance companies who had also made some measurements in the industry. But in terms of the data where we had the same individual making all the measurements, he had to do this on his own. As a result, he didn't get to each mill more than once a year, and sometimes it might be two-years gap between going to a mill.

THE WITNESS: (cont'd.) The other problem when you have an individual who has to do all this on his own, is that you are likely, if you get an increase in a particular area or in a particular year, he may not have the time to go back and make all the necessary checks on that.

What we had done with him, though, was to go through and say we want the values which are the most representative of that mill in any one particular year. I'm not sure one can hang much on this apparent cyclical...

DR. UFFEN: If we were obliged to try to make some kind of a...establish a figure to be used prior to 1950, would it be safe to say that there would be argument, reasonable argument that it might have a very wide range depending on the envelope that would encompass the oscillations in that worst mill?

THE WITNESS: Yes, I think that's true, though, even in...I think it will also be true in some of the lower concentration mills. If you had taken your samples in the bagging area in the early-1960's or late-1950's, and also in the warehouse, you would get quite a big difference in the concentration.

So your range within a mill, within any one year, is very, very, very high, and this is one of the problems of taking overall means, if you like, for a mill. To be comparable from year to year, you really would like to have the same number of samples taken in the same areas in the same way, every year.

You don't get that, and as a result you might end up with proportionally more high areas being measured in one year than another year. These figures have not been adjusted to take account of the number of measurements at each location in each year, to even out the number.

DR. UFFEN: Just so that I can nail down what I

DR. UFFEN: (cont'd.) think is the importance of this for one of our concerns, is that you might be tempted to project into the pre-1950 era something similar to that average of a hundred particles, a hundred million particles per cubic foot. But if you do so, you must bear in mind that there may have been workmen exposed to double that - two hundred million.

Have I grasped it?

THE WITNESS: Yes, yes. Maybe this is a good time for me to perhaps explain how we did our assessment of the exposure of the workers in the industry, for the studies with McDonald, because we were very much aware of that possibility.

What we did, we collected together from the data which LaChance had available for the mining industry, for each year and by work place, we looked at the concentrations on each floor when we had measurements in those years, we looked at the overall mill concentration, we identified certain work locations where a worker would be restricted.

In other words, if he was in a bagging operation, he wouldn't be wandering all over the mill. If he were doing certain other operations, he might be...such as floorman...he would be a highly-mobile individual.

We looked at the concentrations and the pattern, if you like, of dustiness for each of these locations in the mill over this period of time. We then took each of the jobs that each of the people in the industry - that was some twenty-eight thousand employees - where they had worked, and we looked at which locations those jobs would have been associated with - would they have been associated with working over the whole mill, or would they have been associated with these particular areas.

We then assigned for those years, to those jobs,

THE WITNESS:(cont'd.) the concentrations associated with those particular work areas. So if the variation between someone who worked in the open pit and somebody who worked in the mill would be quite different, they would get assigned the concentrations there.

Now, there are some limitations, obviously, which always exist, and it's for this reason that one has to caution about the ways in which one might use the exposure for such a group.

Although we have, in essence, through using this approach, assessed an individual exposure index for each employee - in other words, you take each employee, take each of his jobs, assign to each of those jobs the concentration associated with where he works, multiply that by the time that he actually spent in that work location, and we allowed in there for break periods in the calculation, estimation of the number of hours the man worked. We took account of changes in hours of work per day over the period - what we find is that it's possible to look at the index we generate as an individual index.

The danger is, if you...but it's designed to reflect, if you like, what is going to be true of the group. Therefore, to pick out an individual out of that group and say he had a hundred million particles per cubic foot may or may not be right. What we've done, we have assessed for his job the best we can in terms of what the areas in the mills would have had, which were associated with those jobs, and an individual who is working in that area might, through his work practice, get himself exposed well above those, and perhaps in present day situations, because of use of protective equipment, might well protect himself more than that value.

One is always faced with this when we are looking at groups. We have a measurement in a particular location, but

THE WITNESS: (cont'd.) individuals in that location may get quite different levels of exposure by their work practices, by the way in which they work.

What we have, we have assigned those area exposures to individuals. We have done it on the basis of individual jobs, so that's better than taking the total mill and saying they all got that.

At the same time, when you come to an individual, he might get himself exposed well above what is the average, if you like, for a particular location.

MR. LASKIN: Q. What assumptions did you make with respect to a pre-1949 period?

THE WITNESS: A. Okay. As far as the pre-1949 period, we sat down with management and with employees and we sat down with a tape recorder and we discussed what was involved in different operations in the pre-1950 period.

We asked employees to make comparisons, if they could, between dust conditions that they knew in the fifties and the sixties with conditions that they knew back in the forties and the thirties.

The general view was that what we had in the early 1950's reflected pretty well, very close back to the turn of the century or at least to the 1910's, the sort of concentrations that they could remember existing in those periods of time.

So we utilized the concentrations back in the early 1950's, in general, to extrapolate back, and modified somewhat by the information we had available from the employee and management groups about what conditions were.

The whole area, the problems of extrapolation become very difficult and I think there's perhaps something which may be worth considering later and...in relation to the

THE WITNESS: (cont'd.) epidemiological data.

I don't think it really matters too much once you get back to the sort of order of magnitude of exposures that were occurring in that pre-1950 period...what the error is on it. You have very high exposure, and that's the group where your health effects are occurring.

What we are concerned about almost certainly for the future, and we have demonstrated the health effects in very high-exposed people, hopefully in the future we won't have such very high-exposed people. So we are interested in more precise measurements of the concentrations down at the much lower end of the scale, and...well, we have attempted to use that in one other situation - I think the study described by Dr. Becklake where we did not have any measurements in one of the industries prior to the last few years, so there we got workers to...we got employees and management independently to assess what concentrations were in certain areas in the past in relation to what they were now, at different locations.

What that permitted us to do was to get some assessment of the low-work areas in the past. But in terms of the very dusty conditions, all we could say was that they were above that in the employees' and management's view, but we don't know what they were. They could have been three times, five times, ten times higher than they were in recent times.

The assessment of dust, the judgement of whether there is a lot of dust in the air or what the concentration is, is very, very difficult, and to get somebody to say whether or not this is one fiber or two fibers per c.c. by just looking at the environment depends on many, many factors.

But when you go back to conditions where the sort of description the workers would give you - well, I used to work on that floor and I could see maybe from about here to

THE WITNESS: (cont'd.) there - we know there was lighting in the mill, and so on, and management - it wasn't a question of workers saying things were bad and management saying things weren't. We had pretty good agreement between people as to what conditions were like, and the dust measurements in the 1930's, 1950's bear out that exposures probably could have been quite high.

Does that answer your question?

MR. LASKIN: Q. I take it you didn't, for example, do what we have seen in one or two other studies - that is, take some factor, take the 1950 or 1955 exposures and apply some factor to estimate exposures twenty years previously, or something of that nature?

THE WITNESS: No. No, no. We based it on essentially what measurements we had in the early-1950's, and we were told by employees and management, well, things were very much like that way before.

There were one or two mills which began, as I said, before some of the concentrations were somewhat lower. We took account of that in that the employees working in those mills would have said to us, well, conditions here would have been like they are now in the dry rock storage bin.

The dry rock storage bin, as you can imagine, the fiber was dry, it dropped into there, there could be a fair amount of humidity, there would be a fair amount of dust, and they would say, well, the visibility was about like that back in the 1920's, or something.

We had measurements in some of those areas which gave us an idea of what we were dealing with.

Q. Were there any external data around? For example, any government data in respect of any periods?

A. Very, very little. In the...I don't recall

A. (cont'd.) that we used any government data for the Quebec asbestos mining study.

We had data from, at the asbestos end. They had had a nurse who had been trained in making some industrial hygiene measurements very early. They had some measurements which went back, with other techniques. For example, they used an Owens jet impinger and various other pieces of equipment which we had a very great difficulty in utilizing this information because we didn't know how it related in any way to any other techniques.

Insurance companies, Travelers Insurance, various other companies, had made measurements in some of the mines and those data were made available to us.

We chose to look at what they looked like, but not... to try not to confuse ourselves by mixing in these other sources of information because we had no way of knowing exactly how well they were done or who did them, or when they were done, what conditions were, whether they were..somebody wanted to demonstrate the worst condition or the best condition.

Whereas with the LaChance data we did, we had the man himself there to tell us what his notes meant.

DR. UFFEN: LaChance kept weekly reports or something, eh? He had these old workbooks or reports of some kind?

THE WITNESS: Yes. The Quebec Asbestos Mining Association will have all of Maurice LaChance's original recording data sheets, and LaChance made available to us, and the industry at the time made available all his files to see whatever we wanted for that study.

Okay. This was to just show that not only in Canada, but in South Africa, there had been some reduction trends in reduction of concentrations, and these concentrations still...this is reproduced from the paper...the same one...

THE WITNESS: (cont'd.) and this illustrates perhaps one of the difficulties of international comparison of data, because their concentrations were measured with a konimeter and with a thermal precipitator, and the midget impinger was never used. The membrane filter was never used until recent years.

DR. UFFEN: Could I ask a brief question here about the thermal precipitator? The name implies that it must dry things out a bit, or use heat or...?

THE WITNESS: Yes.

DR. UFFEN: I'm just curious as to whether the procedure altered the nature of the dust.

THE WITNESS: I would think it's highly unlikely. The principle on which it works..if you look around a light bulb sometimes a ceiling looks very dirty, and this is because as dust particles come near the hot source they get repelled from that surface to the nearest cold surface, and this factor is recognized and somebody devised an instrument in which you have a wire which is heated, and on either side of that wire - it's a very close distance - a glass slide.

As the dust particles pass down by the wire, they get repelled to that surface, so there's not going to be any direct...it's very unlikely that there will be direct particle contact with the hot wire. The particles are likely to be diverted to the cooler surface fairly quickly, and I would think it would be highly unlikely there would be sufficient temperature for breakdown of the material.

DR. UFFEN: And changing the humidity though?

THE WITNESS: Yes, there might be some change in humidity as the air goes past the hot wire. It would be a very slow...the flow rate was very low - a few c.c.'s a minute as opposed to now we talk about liters per minute - which is a very low flow rate.

DR. UFFEN: Is that method used anymore?

THE WITNESS: Well, yes, in some instances it's used. It has been largely superceded by other techniques, but the method is rather an interesting one, because the efficiency for collection of small particles - I'm not talking about fibers - particles, is close to a hundred percent. It would deposit the particles, of course, on a nice, flat surface, so if one were interested in looking at particles, airborne particles, on a flat surface, or you were interested in, perhaps, electron microscopy, it could be possible, and it has been used by the British Medical Research Council even in recent years, to put an electron microscope grid in the same location as the slide.

Now the particles are deposited nicely on the grid in the form that they actually exist in the air. That has many advantages over the techniques of having to transfer particles from one location to another.

So it has applications, but in practical terms it's not largely used. The South Africans might still do some thermal precipitation because they have been interested in the conversions between membrane filter, thermal precipitator and so on. There are a series of papers by Chutoi on that conversion.

Does that answer your question?

DR. UFFEN: Yes.

MR. LASKIN: Q. Were you suggesting just before that we should be wary of comparing dust counts taken, for example between the thermal precipitator on the one hand and the midget impinger on the other?

THE WITNESS: A. I think in terms of saying what the direct relationship between them is, it requires a careful study of the concentrations recorded by the one and recorded by the other instrument.

In other words, the two instruments will not give

A. (cont'd.) you the same concentration, for very simple reasons.

If we just consider spherical particles to start with, if we use the thermal precipitator we would collect close to a hundred percent particles, and because we have now the material nicely sandwiched on our slide, it's possible for us to go to oil immersion for particle counting so we can count down to close to half a micron, maybe even a bit lower, in diameter. Whereas with the midget impinger, first of all we are collecting particles in a liquid medium, and the efficiency for collection of particles in a liquid medium is not uniform according to sizes. So that as we get to smaller particle sizes, the efficiency of collection will decrease because we are depending on an inertial neposition in the solution.

This means to start with we have a lower-than-a-hundred-percent efficiency collection of particles. Secondly, when we do the evaluation on the midget impinger we allow the particles to settle in the settling cell. That means another segment of particles which don't settle in that defined period of time are excluded from observation, and thirdly, when we make the observation we normally count at one hundred times magnification. That, again, gives us some limitation of perhaps around a micron in terms of the particles we see.

So the two instruments do not measure the same thing, so we would be unlikely to expect they would give us the same concentration.

DR. DUPRE: Dr. Gibbs, the surface operations, from that chart, invariably seem a lot dustier than the underground. Is that simply because of the moisture situation underground?

THE WITNESS: No. I think it's because by surface in South Africa, they refer to the milling end of the operation. Their mining operations are underground operations,

THE WITNESS: (cont'd.) and the surface, in fact, are the mill concentrations. So the mill is higher, as it is in Canada.

DR. DUPRE: You have one milling or surface situation there that does seem to be extraordinarily volatile.

THE WITNESS: Yes. I have no explanation. These data are measurements made by and compiled by Chutoi in South Africa, and we put the material together to get the world picture of what was happening in the mining industry.

Any other...?

DR. DUPRE: But if I understand you clearly, in reading that chart I can really read as underground-mining and as surface-milling?

THE WITNESS: Yes, that's right.

This is just a summary of some of the types of instruments that are used in the South African mines and mills. You asked about thermal precipitator. You can see the application where they would consider thermal precipitator as a method. The konimeter has been widely used in South Africa. It's, of course, used in the Ontario gold mines for silica, at one point.

They have a light meter type of instrument which really just reflects visibility change, konimeter which is the same, thermal precipitator, membrane filter, and they have a variety of mass samplers which are somewhat different from the North American mass samplers, which I expect M. Trudeau will have described.

Can I have the next slide, please?

For the calculation of exposure of Quebec chrysotile miners, we used midget impinger data, and we assessed for each individual in each of the studies, so that would be the mortality study, the radiological study and so on, an individual index of exposure, but to be applied for a group

THE WITNESS: (cont'd.) in epidemiological studies.

One of the issues which I'm sure is of concern almost throughout the whole world - the decision to adopt a fiber standard meant that one was stuck with a method, or a series of methods, based on fiber, with no epidemiological data base to be able to support what the standard should be.

I would like to sort of generalize on that, because if one looks at the studies that exist in which there has been an assessment of exposure in terms of particles or in terms of fibers, there are really about four studies, really, in existence where we have such data. The one which, of course, was used for the first standard in 1968, was the BOHS study which utilized data from the Rochdale, industry in Rochdale.

They had used thermal precipitator for collection of particles. Now, the advantage of the thermal precipitator perhaps over many other techniques is that although it will not be totally efficient for collection of long fibers, it will permit you, on the same sample, to do counts of fibers, and with a certain percentage loss which you may or may not know.

Nevertheless most of the...from my reading of what was done, and it's referred to in the British Advisory Panel report, the counts made prior to the early sixties were essentially in particle counts, and a conversion was made from particle counts into fiber counts in the 1960's, based on some parallel measurements that were available.

So even in the data that we have considered to be fibrous information, first the membrane filter technique was not used to measure the concentration, and secondly, some sort of conversion was necessary in order to assess what the exposure would have been in fiber terms.

Now, the pressues, of course, to convert the Quebec data, the McDonald study data in terms of million particles

THE WITNESS: (cont'd.) per cubic foot years to fibers, has been quite intense because it's perhaps the largest population in which there has been some assessment of past exposure.

The problem...can I have the next slide, please... sorry...skip that. Can I have the next?

This is what everybody would like to see - relative risk in relation to fibers per c.c., measured clearly with membrane filter.

Now, can you put the next...well, put this one in and I'll..

This also is in one of the papers you have. This was to show how the exposure for an individual was calculated. I'm sorry we got off the track a bit just earlier.

MR. LASKIN: I think that's at tab six.

THE WITNESS: Can I have the next slide, please?

MR. LASKIN: Seven?

MR. HARDY: Six, page ...

THE WITNESS: Yes.

This information that is here should be very similar, if not identical to, the data in the Dagbert report, in that these are the data points from material which I had let the Beaudry Commission have, and we have plotted here the log of the midget impinger count versus the log of the membrane filter count.

When we finished our first series of studies with the relationship between mortality, x-ray change, function symptoms and dust exposure expressed with the midget impinger, we looked at what were possible ways of arriving at a conversion factor to be able to convert midget impinger counts to membrane filter counts.

First of all, in the Quebec mining industry there were very few measurements of airborne concentrations using the membrane filter, made prior to 1970. We had made a few

THE WITNESS: (cont'd.) ourselves in the late 1960's and very early 1970's, but midget impinger had been continued through until then.

When the membrane filter method was introduced, most mines did not continue for very long with the midget impinger method in parallel with the membrane filter method. There was a certain amount of overlap at some mines, but not for very long periods of time. So the number of data points available where we had side-by-side measurements were relatively limited.

We had made a series of measurements ourselves in the early 1970's, where midget impinger and membrane filter were placed side-by-side at the same location to obtain a ratio, to determine concentration by both techniques.

The Beaudry Commission had requested that we give them the raw data.

Dagbert used this raw data to arrive at a relationship which he describes in his report, between midget impinger and membrane filter.

MR. LASKIN: Q. He used the six hundred and twenty-three?

THE WITNESS: A. He used the six hundred and twenty-three values.

Q. Which were the side-by-side measurements that some of the mines had made?

A. That's right. These were made by the McGill group, at the time. These were not from the mines themselves. These were done by the McGill group.

Q. Okay. But they were different...were they different measurements than the earlier measurements that you spoke about that formed the subject of your earlier paper?

A. Yes. The previous...the first studies, McDonald studies, went...began in 1966, and the dust assessment for the

A. (cont'd.) employees was done to 1966 using the midget impinger method. We had no fiber measurements in the industry until the early 1970's, and we made a special survey in the early 1970's using midget impinger and membrane filters side by side, to get some idea what was happening.

Now, we also published in the early 1970's, the paper with LaChance, based on a series of measurements made just before this.

Q. That's the..

A. The eighty-seven values.

Q. That's tab seven, I think. That's the paper in the Archives of Environmental Health.

A. Yes. In which we showed the sort of order of magnitude of variation of ratios. You can see if you look at different mines in that paper, and you look at different locations within mines, that there is a fair amount of variation in the ratio that you obtain.

The limitations of that study, of course, are numbers of observations, which were also shown are very, very small.

If you look at the diagram in that paper you will see that the points are scattered all over the place. What we found in the larger study was exactly the same thing - the points are scattered all over the place, they are...although one might be tempted to put a line through this recognizing we have already logarithmically transformed it to eliminate those large outliers, one might be able to draw a line equally well this way as that way, so it's a problem.

Can I have the next slide, please?

This gives you the distribution of the...somewhere we lost a point of the six twenty-three...but this gives you the range of ratios that we found out of that survey, and you can

A. (cont'd.) see ratios ranged up to as high as eighty-something-to-one, way below one-to-one.

Now, this became, of course, very worrying - what could be done?

Well, there were a number of factors that were important in this, and some of them are not perhaps that well reflected in the Dagbert evaluation of our data.

First of all, when you get the very low concentrations with the midget impinger, you are unfortunately restricted very much in the way in which you can collect the midget impinger sample. It's a liquid media that you use - normally alcohol, isopropyl alcohol.

In a mill environment, which can be quite warm, you get evaporation. So the sample is normally taken over something like a ten minute period because the liquid begins to disappear. This means there is a limit to how much material or how long you can sample to collect your dust, and when you get to very low concentrations, you will find that your number of particles actually observed, on which you base your midget impinger count, are very, very small. So there's a big error in there.

If you count one particle, if it had been two, the ratio would have halved. So you have a problem on the midget impinger end of the thing.

You also have a problem on the membrane filter side for the low concentrations, and possibly for the very high concentrations, because in the very high concentrations particles will overlap with the fibers, and in the low end of the scale, the number of fibers that you observe per field will be below those where statisticians have told us that we will have a lot of reliability unless we make very large numbers of field counts.

So we run into a problem at both ends. Now, one of the things we found was when this survey was done, that the

THE WITNESS: (cont'd.) midget impinger counts in the 1970's were way below those that had existed back in the 1960's, 1950's and earlier. We did not have locations where we had fifty million particles per cubic foot, or a hundred million particles per cubic foot, where we could measure the fiber concentration at the same time to see what the ratio would have been under those sort of conditions.

Now, what have we done about it?

First of all, our conclusion was you cannot take a single figure and multiply your hundred million particles per cubic foot and make it three hundred or five hundred, or whatever.

What we did do was to take subsequently all the measurements which the industry had ever made, prior to 1977 or up the end of 1977, both with the midget impinger and with the membrane filter, plus the McGill data, the figures we had, and we looked to see what the mean concentrations were, or median concentrations within each work area...by midget impinger and by membrane filter...for the areas where we had measurements by both techniques.

Then we looked at what the side-by-side measurements in those areas - if they existed - gave us, and we used those ratios, the median concentration, for example, in the bagging area by midget impinger to the median concentration in the bagging area by membrane filter, to convert the concentration in particle terms in that area.

Now, this doesn't mean that there wasn't a lot of variation with a particular area in terms of the ratios of membrane filter to midget impinger, but they were quite a bit less than we saw overall. In this we didn't get up to eighty-to-one within individual areas. We had variation, but it was not as large and there was a tendency for us to be able to say that certain areas where fiber might be more likely to be around, the general pattern fitted, but where we had rock handling we got

THE WITNESS: (cont'd.) different ratios from where we got fiber handling, and so on.

So we took all the data that existed in the industry to arrive at an estimate for particular work areas what the ratio would be. Now, we were forced to a lot of generalizations. We had some problems where in areas we had no side-by-side data, virtually no measurements of midget impinger or membrane filter at the same point.

For example, some of the open pit operations, we had no data points, and this meant that we had to make assumptions that crushing, for example, which would be the first mill contact of the raw rock, might be what we would expect, perhaps, when handling that degree of opened fiber or raw rock material.

These figures were then used from each of these locations. We then linked them with each job and the McDonald analyses, the case control studies that he described, the fiber assessments were then done using those figures. So these figures were derived by location, not a single conversion factor.

Now, the possibility existed - and I think this is important - the possibility existed in doing the conversion this way for the conversion factor to totally change the dose-response relationship. It would be impossible that whereas somebody who was exposed to a hundred million particles per cubic foot before, for that number to be multiplied by, say point five, where the concentration now would come down from a hundred to point five times a hundred. Whereas a concentration of somebody else who is exposed to ten million particles per cubic foot, the conversion factor might have been thirty, in which case it would have gone up to three hundred.

So the potential existed for totally changing the response relationship.

THE WITNESS: (cont'd.) Now, the reason that this is important is that we really need to know, if we are using the membrane filter method as a method for control, that indeed whatever it measures does relate to health outcome.

We know that midget impinger counts relate to health outcome, but we didn't know for sure whether or not membrane filter counts really related to health outcome.

Now we have evidence from several studies where there has been a conversion of the British data and the Quebec data, that indeed there is a relationship to that converted figure - even though there are all sorts of problems associated with the conversion. You have to make a lot of assumptions. We have to assume that what we are measuring, the figures that we derive from these recent measurements, apply away back, and we know in the industry that in the turn of the century and up until maybe even until the fifties, longer fiber was more important than shorter fiber, that much of the short fiber, perhaps, ended up on the heaps, and later would be of economic value.

So there has been a change in the nature of the fiber that would have been handled in the mills.

Now, what this would have done to the counts, we have no real way of knowing. So we make assumptions and we make reasonable assumptions based on the best available information we have to arrive at these assessments.

Yes? Sorry.

MR. LASKIN: Q. Just to go back for a minute. Were the conversion ratios that you used for each area, were they derived from six hundred and twenty-three side-by-side measurements?

THE WITNESS: A. No. They were derived...I can't tell you the exact number of the measurements that would have been available to calculate the mean concentrations in the various areas. In - there's a paper by McDonald that has those numbers in

A. (cont'd.) it. It's something like...by the mid-1970's there had been something like ten thousand midget impinger counts made...

Q. Over what period..

A. That would include...from 1949.

Q. Okay.

A. And there had been something like ten or eleven thousand membrane filter counts made since the early 1970's, so...

Q. Wouldn't you need the overlapping...

A. That's right.

Q. ...measurements in order to calculate your basic ratio?

A. Exactly. And I can tell you offhand how many values there were that overlapped in that 1970...in terms of side-by-side measurements I can tell you there were very few that really existed, but in terms of the number of measurements that went into calculating the mean concentration for an area by the membrane filter and by the midget impinger method, I can't tell you offhand. We could get it from the raw data how many data points there were used to calculate the mean concentration or median concentration.

Q. Was it then the mean concentrations that formed the basis for the ratio?

A. We looked at the mean and the median. The reason for looking at both is that in some cases you can get a very skewed distribution of data points. You can have measurements, for example, in a particular area of the mill where ten of the points are between one and three, and some you get one and twenty. If you take a mean, your mean might come out right at the top end of the scale. If you look at the median, the median will give you a better measure of the central tendencies.

A. (cont'd.) So we looked at means and medians to make sure whether we were dealing with uniform distributions or not.

Q. But am I...there is one point I am not understanding...am I correct that the basic ratios would have been derived from measurements in the post-1966 period, because that was the only period in which you had membrane filter measurements?

A. Yes. They were based on the post-1970 figures.

Q. Post-1970 period?

A. Yes.

Q. Then I take it that - is one of the assumptions that you made that the percentage of fiber in a particular dust concentration was relatively stable at the time, and you are comparing fibers and dust particles in the 1970's and is there implicit in your analysis that whatever percentage of fiber there is in a million particles of dust at a particular job site will be the same in 1940 as it is in 1970?

A. Yes. There are no, as there are no data available before 1970, one is forced to make that assumption and I can't provide evidence for or against its reliability.

There have...we did look at our data and there has been some further analysis - I haven't seen the results from the U.K. - we provided to London School of Hygiene, to one of their statisticians there, our data to look at the various ways of utilizing these figures for ratios, and I understand he has looked at the effect of concentration, and we also did take a look at this - the effect of concentration on the recorded ratio. We would have liked to have been able to say from our data yes, all the ratios for each concentration are exactly the same. They weren't, and the number of data points weren't adequate for us to be able to say, well, we believe it should have been five-to-one if the concentration were about

A. (cont'd.) ten million particles per cubic foot, and three-to-one between that, and we don't have the data to be able to do that. So we have assumed - exactly as you say - we have assumed that what we measure today applies to the past, and that may or may not be so.

Q. The ultimate average figure that Dr. McDonald used in his 1980 Lyon paper - three point one four - has that then an average of all of the individual conversion ratios in the various job areas?

A. No. How the figure that you see is derived, the ratio that you see in the recent papers - for the groups that have been analyzed, for example the lung cancer population and their controls, for each person in the study we assessed the fiber concentrations that...well, first of all the dust concentration for each individual. We then converted the fiber concentration for each of the jobs that he had worked on, and determined for him a fiber index.

For the total group studied, we then calculated the mean particle concentration that those people would have had, and the mean fiber concentration they would have had after the conversion had been done, and that ratio of the mean fiber concentration to the mean particle concentration gives you a ratio.

So that ratio is based on the distribution of jobs of the cases and the controls in the study of health. It is not a mean ratio of all the measurements in the industry. It's after application to the population.

So what in essence it says is, in this population of workers with cancer and without cancer, the distribution of their exposure in fiber terms and in particle terms was, in essence, equivalent to us having multiplied particles by three to one.

Nevertheless, some of the midget impinger counts or particle counts in there may be multiplied by thirty to one, and

A. (cont'd.) some being multiplied by point five to one.

But the overall picture is three to one, and maybe that's confusing, but that's what...

Q. Is one of the things you are saying that if you changed the job distribution..

A. You change the ratio.

Q. You change the ratio?

A. That's right.

Q. So you can't use three ...

A. You can't take that and multiply any other study by three. That's right.

Q. The paper that - and I'm sorry it's not in Dr. Gibb's compendium of papers - but it's tab twenty-one in Dr. McDonald's papers, which is his 1980 paper which you may remember. I think he fits one of Mr. Berry's regression equation lines to...

DR. DUPRE: I was going to ask about that, counsel. Can I take it, by the way, that what you have been just discussing in terms of what you call the points in your tab thirty-two paper, the case control approach to conversions, can I take it that yet another paper where I can find the outcome of the case control approach is the Becklake paper, Lyon, 1980?

THE WITNESS: No, there is a McDonald paper, Lyon, 1980, which should have been in my compendium.

MR. LASKIN: Maybe what we should do, Mr. Chairman, because we have had the witness talking for a couple of hours, is take a break and we can all get the paper and we'll know where we are.

DR. DUPRE: Why don't we do that, and rise until 11:30.

THE INQUIRY RECESSED

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THE INQUIRY RESUMED

MR. LASKIN: Q. Dr. Gibbs, because I think we now all have the paper that we marked as tab twenty-one in Dr. McDonald's compendium of papers, but which in fact you coauthored with him, I take it that what is described there, and in particular at page 813, is perhaps more briefly what you were telling us just before the break as to how you did the conversions from particles to fibers?

THE WITNESS: A. That's right.

Q. Then do I take it, for the purpose of this study, which was a case control study, Dr. McDonald then took all of the subjects who are listed at page 812 - basically those persons who had died of pneumoconiosis, asbestosis, lung cancer, mesothelioma, etc., and had matched controls and applied the various conversions to those people to get a fiber exposure picture for each person who had died, as well as for the controls?

A. That's correct.

Q. The means were worked from that, and the three point one four which is the ultimate figure that is suggested in the article then comes from the subjects who were part of that case control study?

A. Yes. This is described at page 814, and I can read you from it:

> "The mean accumulated dust exposure for the two hundred and forty-four cases and four hundred and eighty-eight controls so far completed, is two hundred and forty-eight point eight million particles per cubic foot year. The mean accumulated fiber exposure was nine hundred and five fibers per mil year.

A. (cont'd.) "Thus the average conversion factor was three point one four".

In other words, it's the average exposure of the two groups in fiber and in particle terms on which that ratio is based.

Q. Could you just clarify one point, because I see when you read that sentence there was a reference to four hundred and eighty-eight controls, or I take it - two controls for each case - and yet the study itself appears to relate to only one control?

A. Yes, okay.

Q. Per case.

A. Yes. There will be a subsequent paper - I'm not sure where in the paper now, reference to the controls question is made - in the final study in the original plan to do the total lung cancer, mesothelioma, GI tract cancer and so on, varying numbers of controls were taken for different disease groups...some groups had two controls, some had four controls, some had one control...to try and get a workable number of people to assess exposure, and for example if we had had to do the calculations or assessments of exposure for some eleven thousand people, this would take a tremendous amount of time.

Secondly, we wanted to correct the work histories for these workers. Now, that also had some problems and that will be reported in a later paper.

I am not quite sure off the top of my head whether there were one or two controls in this analysis. Can you show me... I see, you've got two hundred and forty-four and two hundred and forty-four there. I would need to check back in here now on why only the one control is included.

But this paper was put together in order to present it at the Lyon conference, and it's a preliminary report. It's

A. (cont'd.) not a final report and the intention was to do a complete analysis, including all the controls.

Q. Can I also ask you a number of questions in this area, but can I also ask you how Dagbert's study fits in with all of these issues, and I have in mind two separate statements about Dagbert's study - one which is found in the Simpson Report, and I'm not certain whether you are familiar with it or not, and the second is a comment of your own which appears at tab thirty-two of your material.

The reason that I ask you is that the Simpson Report seems to have placed some reliance on Dagbert's work for the Beaudry Commission.

Let me just assist you...let me show you page eighty-five of Dr. Acheson's report, paragraph eight. Perhaps you could examine that in light of your comment on your own statement which is at page 173 at tab thirty-two...which is your survey paper.

A. Yes. What page in thirty-two?

Q. 173.

A. Yes. As I mentioned earlier, the Beaudry Commission had asked me if I would make available the six hundred and twenty-three side-by-side measurements to Dagbert for his analysis of the relationship.

When we made the surveys, uneven numbers of measurements were made at each location. In other words, we did not make ten or twenty measurements at each bagging area in each of the mills, at each drying in each of the mills, in each crusher, and so on throughout the industry, and when you start looking at overall factors, the distribution of locations within a particular operation where you are making measurements will reflect the overall concentration.

In other words, if we are looking at a mill mean

A. (cont'd.) concentration, for example, we would find - if we took all our samples in the bagging area - something quite different than if we took them all in the warehouse.

Now, in the same situation because we find large variation in ratios between different areas of a mill, and between different mills, the total composition of the measurements is important in terms of the overall ratio that we get.

Now, I can illustrate that fairly well. Say, for example, most of our measurements were taken in the mine which gave the very high ratio - we had one mine which gave very high ratios of fibers to particles, and this is a slip fiber mine and nobody quite understands why the ratios are so high there, but they are high. Now, if we had made all of our measurements or eighty percent of our measurements in there, and only ten percent in the rest of the industry, we would have concluded that the ratio was going to be way on the high side.

In contrast, if we did it the other way around we would consider all the ratios on the low side.

The danger is in putting the total package of results together without making some adjustments for how you are going to deal with it. So what we did in our latest conversion was to take each mine and each area within those mines to get those ratios.

Now, that doesn't eliminate all the problems, but it's an attempt to overcome this problem of having uneven numbers of data points which might interfere with the observed ratio.

The other problems that exist, and we might well mention there are some complications that have to be considered, and I don't think that anybody has adequately been able to take account of them, first of all in the early midget impinger counting Maurice LaChance and other people making midget impinger counts would have used a special machine which has a little handle on it, and he turns the handle. The midget impinger

A. (cont'd.) is designed so that if the pressure drop across the midget impinger is twelve inches of water, the flow rate through the machine is point one cubic foot a minute. This is the flow rate on which the midget impinger calibration is based.

Now, in the late 1970's, late 1960's, personal pumps came on the market. These personal pumps greatly facilitated the work. Instead of having to crank the handle, you just connect the pump up.

Nevertheless, some of the early pumps, and possibly some of the earlier pumps and even today some of the pumps that are on the market, may not deliver twelve inches of water guage across the impinger, and at the same time the flow rate that is used is not two point eight liters a minute or point one cubic foot per minute, but may be somewhat less than that.

Now, the problem with that is that now what you've done is that you have decreased the efficiency or changed the efficiency of collection of your particles - not only changed the volume of air going through, you changed the efficiency - and we have no way of knowing...this came to our attention when we started doing our surveys and we found that technicians out in the field would, if they couldn't get the two point eight liters per minute, might run it at two point seven or two point six, and record that flow.

Now, there's an error associated with that, and we don't know exactly what that error is in terms of fiber efficiency or particle efficiency collection.

There is another problem which produces another variable in this whole...

DR. UFFEN: Before you go on to another one, could I ask a quick question about this? This seems to be an appropriate time.

DR. UFFEN: (cont'd.) When there are very small numbers of particles or fibers to count and the statistics are terrible, could you whomp up the flow rate intentionally by a factor of three or four or five?

THE WITNESS: Not on the impinger, no.

DR. UFFEN: Could you do it for the membrane filter method?

THE WITNESS: Okay. There has been some work done on the membrane filter method to determine the effect of face velocity - that's the velocity of the air going through the face of the filter - on collection efficiency, and the problems, some problems exist at the very low rate end. There probably are less at the high flow rate end, but I don't think there are adequate data available to be able to tell us how high a flow rate we could go without problems.

The low flow rate end, Beckette had found, for example, if he put filters with zero velocity through the filter he would collect fibers on them, and then as he increased, his change was not commensurate with the increase in volume at the low end.

Rendall in South Africa, I think has done some measurements on this type of thing. So that was the reason why in the Asbestos Information Association document there were recommendations concerning the sort of flow rates that should be used, because people had reported outside that range some problems of not knowing exactly what effect they were having on the collection efficiency.

It's equally true, of course, that if you started using these sampling procedures in underground mines where you might have increased velocities of air along drifts and so on, there you are into a problem of possible nonisokinetic sampling of your filter because your air speed can be quite considerable

THE WITNESS: (cont'd.) in relation to your sampling rate, and that would again - orientation of the man and so on - would affect your recorded concentration.

We haven't got that sophisticated yet to be able to take account of those variations normally.

MR. LASKIN: Q. I think you were going to address a second problem.

THE WITNESS: A. Yes. I think the other problem on the midget impinger question is the settling time. The way in which midget impinger counts are made is to allow a certain volume of liquid in a settling cell, and there are different sizes of settling cell, but most people would use a standard cell as set out by the - I think it's the Textile Institute recommendation for impinger counting.

A certain volume of liquid is put in and the particles are allowed to settle, and the time that they settle depends on how many and what size of particles will have reached the bottom for you to observe, because you don't observe the particles falling in the liquid, you observe the number of particles which have fallen onto the bottom of the cell.

The settling time in the counts which the eighty-seven values which are published, the settling time was exactly the same settling time as Maurice LaChance normally used for impinger counting, and which we understood to be twenty minutes at that time...because he did the counts himself in that series.

In the subsequent six hundred and twenty-three measurements we have, the settling time would again have been twenty minutes, in line with LaChance, because our technicians who did the counts did comparisons with him and they did some of the counts together, so the technique which was followed was the same. Nevertheless, when we started compiling all the midget impinger and all the membrane filter counts together,

A. (cont'd.) I had some discussion with Geoffrey Knight at that time, from the Department of Mines, who was working in Thetford with some of the asbestos industry, because he was reporting the settling time was thirty minutes, used by people doing the midget impinger measurements in the industry.

We never totally resolved that question, but its implications are that it could lead to perhaps something like thirty percent underestimation of the particle count for twenty minute settling as opposed to a thirty minute settling.

Now, the data we have, we haven't had a way of separating who did what because it was never recorded. People said we've counted them, and we've included all the data together, but these are some of the practical issues that are behind the scenes when you sort of look at any dust data.

Q. Just on that, can you give us...I want to take it in two stages, but I suppose the first stage is, can you give us any sense of how good you feel the raw dust data is that McDonald's study worked with, perhaps even relative to other epidemiological studies?

A. I would think it's probably far better than most epidemiological studies in the sense that even though we don't have measurements every day and every year and at all locations, we do have essentially systematic measurements made by the same individual over a long period of time.

In most epidemiological studies, many epidemiological studies, we have to search around a lot to find the data, and very often the measurements have been made by a variety of different technicians on different occasions, and the interpretation of that information to calculate your exposure is somewhat limited.

So in that sense, I would say that the data base for the dust assessment isn't bad. In terms of the fiber conversion question, there we are severely limited.

A. (cont'd.) Now, arguments will be made that the total number of measurements made over that period of time is somewhere, if I remember, around five thousand measurements, something of that order, to represent something like ten mines over a period of twenty years. It doesn't add up to a lot of measurements per mine per year, and so on.

Nevertheless, in the distant past, in the early-1950's and so on, there was adequate information to demonstrate that we had a lot of dust around then, and those people who were exposed in those high dust concentrations are in the top end of all the McDonald papers. Those are the people who have gotten high dust exposure with an increase in risk.

So I think the data base for the dust exposure isn't bad. It's probably as good or better than one would find in many other situations, because normally you would ideally like to collect your data yourself, for a prospective study. When you are looking at a chronic disease study like this, and if we want to make some comparisons, you could ask how good is the data base for the Ontario uranium miners, how good is the data base for the Ontario silica exposure, and here we've had again government and industry making systematic measurements. We run into the same problem, but there we've had many people making the measurements, not one individual.

Q. If I read your articles which you either authored or co-authored in the early seventies correctly, you seem to suggest that, at least in the mining industry, and at the stage of your own research in trying to convert particles to fibers, that you would still be better off regulating in terms of dust rather than fibers. Have I put that part of it fairly?

A. I don't think we were quite saying that. It may be that's the way it appears to read in the articles.

What we were saying is that if you intend to set

A. (cont'd.) a standard and you have some data which show that your health effect is related to some index of exposure, then to go off and set your standard on something that you don't know whether it's related to the health effect doesn't make sense.

So for the asbestos mining industry we had good data, fairly good information, which showed that dust, total dust exposure, was related to mortality from lung cancer, it was not such a good predicter of radiological change, but it was related to radiological change. We did not know whether fiber counts did or did not.

Obviously, that's a crucial question if one is going to set a fiber standard.

In practical terms, in terms of measuring with the midget impinger in the long term, no, it's not a good instrument, it's not the instrument of choice to set a standard for the asbestos mining industry. But the philosophy still stands that one should use some index of exposure which we know relates to the health effects. Otherwise, we've no guarantees that down the road, even though we've changed that parameter, that in fact we are going to change the health risk.

Q. Have you had any opportunity to look at the attempts by other epidemiologists or other epidemiological studies to try this conversion effort, and I'm thinking, for example, of Dr. Dement's paper?

A. Yes. Yes and no. As far as Dr. Dement's paper, I did meet with a number of other scientists and with Dr. Dement to look at his data to see whether there was any explanation for why his risks in relation to exposure seemed to be so different from all other studies. I think in conclusion we really didn't find any explanation in terms of conversion. There is ...as the responsible factor. My feeling was that his conversion might

A. (cont'd.) have been a bit lower than I would have perhaps guessed. He has used a conversion factor which is perhaps a little bit different from the factor which Eyre and Lynch had reported. Eyre and Lynch had reported ratios of fiber to midget impinger, membrane filter and midget impinger, in the textile industry.

Nevertheless, Howard Eyre was at this meeting and was able to look at Dement's data, and he felt that under the circumstances that was reasonable.

So I've not had the chance to rework Dement's ratios. I've not had the chance to rework his epidemiological data, but certainly in terms of conversion I don't think that... we would need a much bigger error in the conversion, probably, to explain what has happened. It doesn't mean it's not impossible, and it's certainly something that one has to look at because the implications are that either you've got a very special situation in the Dement case, or that you've got a number of other studies where the exposure relationships are all wrong, or at least they are not the same.

Now, I think if we look at health risks in other parts of the industry, it is possible that you might have major differences between occupational groups.

Now, the Dement one is a particular problem perhaps, in the sense that his population has been reported to be exposed to chrysotile only, and even though there is still this question of how much dockyard exposure and other exposures might have been involved, I'd leave that Dr. Dement to respond to himself. But if you look at the difference between the chrysotile miners and the New York insulation workers in terms of mesothelioma risk, there is an obvious difference.

If you look at the gas mask crocidilite workers and the Quebec chrysotile miners for mesothelioma, there is a

A. (cont'd.) distinct difference.

So there are differences between occupational groups. They have normally turned out to be confused because of fiber type differences. In this case, we don't have that.

I can't give you an explanation at this point for why Dement's information is so different from the chrysotile mining experience, or from the Rochdale experience.

DR. DUPRE: Just to refresh my own mind, Dr. Dement's rough conversion ratio had turned out to be...was it one million particles per cubic foot yielding three fibers per cubic centimeter?

THE WITNESS: That's how I recall it, but I would need to check in the...

DR. DUPRE: That compares...now, is your three point fourteen conversion factor that is in the McDonald figure, is that the same...is this again the million particles per cubic foot to fibers- three point one fibers?

THE WITNESS: Yes, it is, but it's the average based on the occupational distribution of the people in the study. It's not a single factor that one could multiply the total exposure in particles by.

The individual ratios that would have been applied in arriving at that are between point...I think it's point three and thirty.

MR. LASKIN: Q. Thirty?

THE WITNESS: A. Yes, point three and thirty have been applied to individual occupations, and the overall mean for the distribution of those occupations comes out to three point one.

DR. DUPRE: One expert witness we had last month, or longer ago than that, Dr. Weill if I remember right, had, like so many others, been puzzling over the Dement results, and if I

DR. DUPRE: (cont'd.) recall correctly, Dr. Weill, without in any way pretending to be an expert on conversions, simply pointed out that if the Dement conversion ratio had been one million particles per cubic foot translating into nine fibers per cubic centimeter, the Dement curve comes way down in terms of dose response, and is much more in line with the Rochdale study.

THE WITNESS: I'm not sure that there is a simple answer to it. The only way would be to go back through in detail all the Dement conversion data.

The study that was done by Eyre and Lynch back in the late-1960's, if I recall, came out and said there was large variation between individual operations and between different plants, but their best guess was around six to one, if I recall. They said six to one, but like us they said, we don't recommend multiplying things by six because there is so much variation within individual...now that's a little bit higher than Dement's three to one and I think certainly to clarify the situation that conversion had perhaps to be looked at a little bit more.

But he will have the same problem that everybody else has in terms of limited data to arrive at a reliable conversion factor.

DR. UFFEN: Can I try something on you to see whether I've got this correct? The thing that's bothering me is, if in the past some of the men had received exposures that were measured in particles that were ten times higher than the best mill - remember your diagram - I use ten as a round number, and that the conversion procedure used to get this figure three point one four was averaged, that it was based on the number of people and their jobs and was not an average.

But the epidemiological data about people who were ill or died are the ones that presumably got the worst doses. So what we should be looking at is not an average which has included

DR. UFFEN: (cont'd.) the people who were in good locations, which brings the average figure down to three point one four. We should be looking at a figure that represents the ones that are most likely to have got ill.

THE WITNESS: I think...

DR. UFFEN: I may not have worded that very well, but...

THE WITNESS: I think maybe I should go back...it may be that this three point one four question is confusing for people. It would be better, perhaps, if it weren't even in the paper.

DR. UFFEN: It would be better if it wasn't three point one four. If it was three plus or minus six...

THE WITNESS Let us look at how that figure was arrived at again. For each employee in that case control study, we had a work history. For each of the jobs that the man worked on, we took each job and we looked to see which areas that would involve exposure, and how long overall exposure was in those areas.

We had a dust concentration for that area, based on a compilation of dust exposure, so we said - a man worked for three years in twenty million particles per cubic foot, and that became - were multiplied together and for the previous job, exactly the same, and we summed that over the total man's work history.

For each of those work locations we also had a factor based on our conversions. That may be point three for a grader and it may be one for a bagger and two for a mill superintendent or something.

We then multiplied each of those individual exposure factors by that number, so if the man worked for two years as a grader at ten million particles per cubic foot, he got two by ten, by point three, for that component.

THE WITNESS: That was then summed over the man's total work history - now in fiber terms, and the figure that's given in that paper is the average of the accumulated exposure in fiber terms, and in particle terms, for all the people in that study. That figure isn't used to multiply anything else by. It's not a figure that we...so it's not...

DR. UFFEN: No, but it's the one that we tend to compare other studies like Dement's with this, and I don't...

THE WITNESS: Yes, but it's not correct.

DR. UFFEN: ...think they appear comparable.

THE WITNESS: No, they should not be compared at all. That figure is derived from the actual exposure ratios and from Dement's three to one we are talking about, we don't have that same figure. It could be derived almost certainly, because he should have the total exposure in fiber terms and in particle terms to get back to it. But he has used three to one overall, whereas we have used the whole range of ratios to get that three to one overall.

DR. UFFEN: If I exaggerate just for a minute, to make sure I understand, suppose I took a study like Dement's and I included another hundred people that had low exposure histories - like the front office people or the bus driver or something like that...

THE WITNESS: You wouldn't change anything.

DR. UFFEN: ...and I worked out the results in a manner the same way as you did, and then I found an average for that new situation, I would get a lower figure, because it would have included a whole lot of people with low exposure history.

THE WITNESS: Well, no, you shouldn't get a lower figure because your fiber conversion will be that low figure multiplied by his three, still. So you should still get three.

DR. UFFEN: I see.

THE WITNESS: I think you should still get three, because he applied a single figure - as I understand it - to the total...

MR. LASKIN: Q. Dr. Dement?

THE WITNESS: A. Yes.

Q. But in terms of that interchange and in terms of what you have been saying, I have some trouble with - if you look at page 814...

A. This is the...

Q. ...of the Lyon paper, the McDonald paper, you say in the last sentence of the first paragraph:

> "In the absence of evidence to the contrary, the average conversion factor may be usefully applied to other results in this industry that have been published previously".

A. Yes.

Q. Now, if I understand the dialogue that has been going on, we should only be using this conversion factor for the precise job distribution that you had in the case control study?

A. Yes. When this sentence went in, Corbett McDonald and I had some interesting discussions about the implications of that statement. My preference would have been to have restricted that usage only to that particular situation.

The arguments that went around that is that we have a number of other mortality studies with lung cancer involving occupational distributions which are not unlike that in the case control population, and the three to one is not going to be far - and the three point four one or three to one - is not going to be too far out in looking at those data.

A. (cont'd.) But my preference was not to have that there and that's what happens, I guess, when you have multiple authors in any paper - somebody had to compromise. My preference would have been not to have it there.

Q. They say that judges often have the same problem when they go to write judgements.

Dr. Mustard?

DR. MUSTARD: Can I enter into this discussion about Dr. Uffen's question, which addresses, in a sense, the other side of the coin that has been matched in to exposure measurements.

The cohort of people that are studied is important not only for the estimate of exposure, but it's also important for the estimate of outcome. Therefore, as we know, I think quite clearly, if one loses cases in any study of this kind, that's always a worry that you didn't track everybody down, because what in fact happened to them.

But I think Dr. Uffen placed in my mind the other aspect of the question as well, that if you take a cohort of workers that were working in a certain kind of plant - let's say they all worked in that plant at a certain exposure - if we look at their outcome we may get ratios of observed over expected mortality which are five to one, four to one depending on circumstances. Then when you go into another environment where the works have been exposed, but in a different kind of environment, you may find the ratios with that cohort are two to one or three to one, something like that.

I think one example that comes to mind for me in records that we've seen here from the Johns-Manville plant in Scarborough, we are looking at these ratios that are quite high in some areas in comparison to the material we've seen from the Quebec mines.

DR. MUSTARD: (cont'd.) I don't know how to interpret all this, but it does suggest that within that environment the work force and their exposure is different.

In other words, the outcome side is different, and so the problem one has is, if I can put it this way, when you have the uncertainty about defining what people were exposed to and what they are really like as mixed up and the outcome measures are mixed up, if in your cohort you have a number of people who didn't really have the same kind of exposure as a more tightly-matched group, then you try to match these measurement data into it, you have deluding effect on the outcome measurements as well, which compounds the problem.

Am I getting this quite clear, Bob?

DR. UFFEN: Yes.

DR. MUSTARD: I wonder if you had any chance to take a look at that kind of problem in different settings. I don't believe you've seen or had a chance to read Dr. Finkelstein's paper in the Canadian Medical Association Journal, which is one of our exhibits, or not.

THE WITNESS: I think there are a number of problems in making any comparison between one group of employees, industry, and another. There are some very basic ones we can think of. One is, the appropriateness of an external population to calculate an expected is a real one.

If you look within a province or across the country, there are some marked regional differences in rates of certain diseases, in mortality terms. So you may happen to have an industry in an area where, for some reason, whatever it is, there is a high rate of something or other, and your comparison then presumably should be whether or not these people are higher than the rates of the community in which they exist.

The data often are not available to adequately

THE WITNESS: (cont'd.) look at that.

There is the other problem of regional differences in diagnostic tendencies. Autopsy rates can be quite variable between different regions and between industrial groups and the outside.

For example, if you have an industry where you think lung cancer or some other health effect can only reasonably be diagnosed by autopsy, you might well find a higher rate of autopsy in your employees than in the community.

That means your chances of finding the effects go up, and a very good example of this, if one takes the McDonald paper, 1970, I think it is, if we look at the autopsy rates there in relation to exposure, you find the autopsy rate is much higher among the highest exposed than among the lowest exposed.

Now, is that the explanation for why you are finding more lung cancers or not? We don't really know.

So there are a number of problems associated with the comparisons.

Sorry, did you want to...?

DR. UFFEN: I think I can tidy up my original question a little bit better and put it in another form.

When you worked out the exposure history for an individual, and you used the fiber/particle conversion rate appropriate to the work place where he was, were there significant differences in those individual conversion factors for a man working in one part of the mine or mill and a man working in some other part?

If my memory is correct, they varied by factors of up to thirty. Did I get that right?

THE WITNESS: That's right. Within a single mine I don't think, there may be one mine where it may have ranged

THE WITNESS: (cont'd.) as much as that. Generally the range would not have been as high as from point three to thirty, but it would have been significantly different from one location to another.

DR. UFFEN: Then in my artificial example, if you dilute the cohort with a lot of people from a part of the operation where the conversion factor for that part of the operation was an extreme one, it would then have an ultimate effect on that average you get?

THE WITNESS: You are quite right, yes.

DR. UFFEN: I just wanted to tidy that up.

THE WITNESS: You are quite right.

MR. LASKIN: Q. Is the breakdown of the data that ultimately led to the three point one four figure able..is it to be published, or has it been published?

THE WITNESS: A. No, it hasn't been published yet. Originally we had hoped to present it for the BOHS conference last year, and the stimulus to get it all ready in time for that meeting would have probably been that it were accepted for presentation at that conference.

As it so happened, we submitted an abstract and they said they...I think they had a lot of asbestos papers and they decided not to. So we have not put the material together in a published form, and my leaving the university at that time has meant that my time has been limited in getting this material together for a formal publication.

That was our intention, to have formal publication on the details of the methodology that went into that conversion.

I don't know whether one will learn a lot more once that has been published, but the...as I say, the raw data have been made available to London School of Hygiene to try and see what they get out of it, and whether they can perhaps be

THE WITNESS: (cont'd.) more optimistic about making some adjustment for some of the past higher concentrations.

But I'm doubtful myself. I think we've got the best we can get and we may refine it a little bit.

There is one thing, though, that is worth thinking about, and I don't know whether this is the point to introduce it, but I think it's an important factor. Corbett McDonald might have mentioned it, and possibly others might have mentioned it.

When you look at cumulative dust exposure in relation - as the exposure component in relation to response - most of the variation in the dust exposure is not due to the dust component, it's due to the duration component.

Now, if we go back to the early...for people in the mortality study exposed in the early fifties and before, the range of variation we have for their exposure is something like point one to a hundred million particles per cubic foot - something of that order.

So we have a very large variation, potential variation in exposure.

As we come down in concentration to where people are exposed to maybe five million particles per cubic foot, or below, but can be exposed for up to thirty, forty, fifty years, now within the years duration of exposure you have a fifty-to-one factor. In your dust exposure, you only have a ten-to-one or five-to-one difference.

So when you start looking at exposure dose-response relationships, increasingly in the future you won't have the same order of magnitude of variation in dustiness. It will become more difficult to distinguish what the effect of the dustiness is from the duration component, and already in the past data, duration is the main contributor to the differences that you see, that duration out of that total dust index plays a major part.

THE WITNESS: (cont'd.) I thought that is worth mentioning because we spend a lot of time worrying about the absolute order of magnitude that the past, the very distant past dust exposure. We know we have problems, we then come down into the more recent ones, and duration is a confounding factor, if you like, in trying to sort out exactly what is going on.

There are ways around it, methodological ways around it, by matching durations of exposure in cases and controls, and so on. But they haven't been done. I think there is a paper by Berry, I think, where he has attempted to do something like that. Normally, it's not done, and it is a problem getting rid of duration effect and age effects and others.

I would add perhaps one other thing to Dr. Mustard's question about how does one eliminate this problem of having a certain small segment of the population exposed to something different within your group, that might affect the outcome.

In the case control study, of course, the attempt was made to make sure that age was not a factor, by matching for the same age. The person had to survive to the same date as the person who died so that his potential to be available for exposure was the same.

You did not want to match on exposure, because then you would not see any exposure effect. In other words, you had to allow that component of the thing to be free, and it would be conceivable that the distribution of occupations in the controls and the cases might well be different, because we are looking at a, as you are getting a higher exposure among cases, then they must have had different occupational distributions from the control and that then brings you down to - is there anything associated with particular occupations that gives rise

THE WITNESS: (cont'd.) to this increase risk or not.

At this point in time, the belief is that it's the level of exposure that is responsible for it and not the individual jobs, or something special about those jobs that is giving rise to the effect.

That interpretation could still exist. I don't think it would be that tenable, but it would be one possibility.

MR. LASKIN: Q. Could I just ask you one more question, perhaps, before lunch? We've been told in evaluating the data out of Rochdale that we should be making some adjustment for the fact that sampling measurements in the past were done by area, and now there is personal sampling and indeed you are probably familiar with the paper where that adjustment was made.

In evaluating the Quebec data should we be making any similar adjustment?

THE WITNESS: A. It's a very interesting question. The material you refer to is the Steel comments in this report, and I think that there is little doubt that there are changes in the approaches to measurements - or have been changes in the approaches to measurements - with the membrane filter over the last twelve years, which would lead to probably a high increased reported concentration today for the same concentration one would have had back in 1968.

In other words, if you measure the same concentration today by the one 1968 method, and by today's method, today's method is going to give you a much higher number than the 1968.

What are the reasons for that? One is related to this question of personal versus area sampling. There will be differences in opinion on what the effect of that is, and I don't know whether we have adequate data - it's possible that there might be some data from our surveys in the industry - to

THE WITNESS: (cont'd.) look and see what sort of effect that would have, the difference between area and personal monitoring.

Generally, we would find that personal monitoring gives us higher concentrations than location samples, but it's another area where generalizations are...you look at the overall picture and the overall picture would be that personal monitoring would give you something higher than location monitoring.

If you start looking at specific locations, you would find some specific locations probably where that didn't happen. That's because of the nature of the job.

In some locations it is possible for the person to get himself exposed more than the general concentration. In other locations, it's not so easy for that to happen, the nature of the exposure as such. But yes, in general one would think that personal monitoring would give us a higher value.

Secondly, the early counting with the membrane filter in Europe, and possibly in some of North American centers, but most did not, was to count full field counting, and Beckett demonstrated quite clearly that if you counted all the fibers in the total field, then you had a gross underestimation in comparison with what you got - I think with factors around two to one, something like that, factors two and a half to one maybe - for counting by the modern technique of counting down the microscope, and the earlier one.

There is additional changes that have taken place. We have improved the quality of the technique for clearing filters, and in fact there are some differences reported between acetone clearing and triacetone alone clearing, so depending on what technique you use for clearing your filter, you can get different counts.

You can find increases in this, and variations in

THE WITNESS: (cont'd.) concentration would have occurred because when fibers were first counted under the BOHS technique, the fibers which entered the top and sides of the fields, if they entered the counting area were counted, and the ones that entered the bottom sides were not counted, and some further evaluation that, almost ten years later, revealed that this produces a bias. So that you have a bias in your counting procedures.

So now the technique is being revamped so that you count each fiber entering the field as one half, and on average then your count statistics come out in an unbiased way.

All of those factors add up to the fact that what we are counting today has increased our concentration, observed concentration, over what it was then. One can argue about the order of magnitude of it. Possibly Steels, I've heard figures like Steel's discussed in other settings, and maybe one is talking about something like four to one, maybe overall three, four to one change.

I don't have the data for the Quebec industry to say that that is what has happened. I suspect that if one went back and took all the fiber counts out - there have been a lot over the last ten years - one might find some way of getting at the question.

There is also, though, a serious problem of counter that exists in any situation. From the early counters a lot of assumptions were made that somebody could learn to count fibers very, very quickly. When Beckett first produced his paper which showed that they had a fair amount of variation between different counters and there was a need for training and ongoing collaboration, and I can think of..(static and loss of sound for two seconds)...in the records. If you look to see whether they really were measuring the same thing, one is never quite sure.

MR. LASKIN: Perhaps it would be a convenient time to break for lunch, Mr. Chairman.

DR. DUPRE: Shall we rise until 2:15?

MR. LASKIN: Sure.

THE INQUIRY RECESSED

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THE INQUIRY RESUMED

DR. DUPRE: Ready, Dr. Gibbs?

THE WITNESS: Yes.

DR. DUPRE: Proceed, please, counsel.

MR. LASKIN: Q. Just before you turn to another topic, Dr. Gibbs, my friends at the counsel table just all want to make sure we understand one point which related to the last question I asked you just before lunch, which in terms of whether there should be an adjustment for area as opposed to personal sampling.

Can I just understand in terms of your own measurement that you did in respect of the Quebec situation, were you doing location sampling or personal sampling?

THE WITNESS: A. Oh, all the midget impinger data that we used in the industry were area samples. For the conversion we also...for the conversion six twenty-three values we used location samples in order to get side-by-side measurements.

For the membrane filter counts that are used in the calculation of the averages, there is a mixture of counts which were performed on personal sampling and at locations, with an increasing tendency in recent years to use more personal sampling. But the majority of samples were made, were location samples as opposed to personal measurement.

So my feeling would probably be as far as an

THE WITNESS: (cont'd.) adjustment for that factor, it probably would not be called for - to make an adjustment because we have location against personal - unless you are setting your standard in terms of a person sampling. Then you need to know exactly what technique you are applying and so on, to make that adjustment.

As far as changes in the count level, I'm not sure I have the data - I haven't analyzed the data in a way to be able to respond to whether or not you should adjust the figure for a count level.

Q. Thanks.

A. Earlier I had shown concentrations for... earlier I had shown you the concentration for the - in million particles per cubic foot - in the mid-1970's for the New York meeting. I had a look at what were the concentrations in the Quebec mills, based on the fiber concentrations that were available, all the fiber concentrations available to me to the end of 1977, and this was to illustrate again the best mill here in fact is the same mill that was the best mill in terms of particle counts. The worst mill is not the same mill as was in the particle counts. That mill is now out of business, it's closed down.

But these figures are not converted figures. These are concentrations as recorded in the industry.

The idea of showing it is to illustrate that even in recent years our fiber concentrations in, and the number of samples in different areas and so on - I've explained in the text from which this comes - are not the same, so direct comparability is difficult.

Nevertheless, we indicate that the order of magnitude of fiber concentrations in that industry were fairly high, and a lot of people have expressed perhaps surprise that fiber concentrations could be, you know, twenty, thirty, forty,

A. (cont'd.) maybe hundreds of fibers per c.c. in the industry.

When we look at more recent figures where fiber measurements have been made, we can see that even in recent times there have been some relatively high fiber counts in the industry...which makes us perhaps a little bit less surprised that when figures are converted they do produce high figures.

MR. LASKIN: Just to identify that slide for the record, it appears to be taken from page 172 of tab thirty-two.

THE WITNESS: As we are still on this question of conversion, I thought it might be useful just to give you some indication of what concentrations and figures look like in the textile industry.

These figures are...I don't think they are part of the paper by Becklake and Gibbs, but they are part of a report to the head of National Health and Welfare.

The figures shown in this slide are part of a report to the Department of National Health and Welfare, which is part of the final report for the grant we had to undertake the work reported by Becklake et al on the manufacturing industries.

You have a copy of that paper, but you don't have these figures. That could be obtained for you, or you could obtain from the Department of National Health and Welfare a copy of that report.

MR. LASKIN: Q. Are these figures taken from measurements at that textile plant in Montreal?

THE WITNESS: A. That's correct. Yes.

Now, the ...sorry.

MR. LASKIN: I'm just going to identify that. I'm sure it's the...well, carry on, Dr. Gibbs, and I'll find the paper in a moment.

THE WITNESS: Okay. In this particular plant we had no measurements other than a few fiber counts that had been conducted by the Quebec Government in very recent years, and we were interested in trying to reassess the fiber exposure in this plant, and as part of that a survey was carried out in the survey measuring - using a midget impinger and the membrane filter - and there were no midget impinger results in that plant to convert, but we were just curious to see what sort of order of magnitude we would get for a textile plant as we had some data for the mining industry and also from another manufacturing plant.

You can see here that - two things - one is the question of how different is the personal sample from the location sample, the stationary sample, because in the righthand column we have the fiber per milliliter by person sample for people working on blending, and also the membrane filter count at the location. We also have the midget impinger count in million particles per cubic foot.

Now, you can see that here the midget impinger counts are all very, very low, and because they are very, very low the number of particles that are actually observed when the concentration is measured over that short period of time is subject to fairly wide error.

At the same time, the ratios here would look like almost ten to one - maybe in this one case here we've got twenty-five to one, six to one - here on the mean it's less than one. This gives you sort of an order of magnitude of the variation that we are seeing in a textile plant where the concentrations are around the five, six fiber per c.c. level.

These measurements, of course, are done in fairly recent years.

MR. LASKIN: Q. This is a chrysotile textile plant?

THE WITNESS: A. This is a chrysotile textile plant.

A. (cont'd.) Now, we did have some problems in this particular plant because the Quebec government had been in to the plant and had made some measurements, maybe very small numbers of measurements, at different locations. But it was not quite clear how representative their samples were and they weren't sure either. But the union at the plant said, there is something wrong with the McGill numbers because they are lower than the government numbers, and the company said, there's something wrong with the government numbers because they are higher than the McGill numbers.

So we arranged with the Quebec government to go back and make some comparisons with them, and we took both personal samples and stationary samples again, and here these samples were counted by both laboratories and I do have...I don't have it here... in the report there is a comparison of the results from the McGill and from the SPE, and we were very, very close in our concentrations reported by the two groups.

Now, what this meant was that we were seeing by taking surveys on different times and different periods the sort of variation which occurs with time with approach to sampling, because in our second study we standardized on how we were going to do everything. The samples were then split and they were counted independently by the two laboratories.

But here you can see that the personal samples, if you look at the median, is about three to one, here it's about two to one, here it's about one and a half to one, here it's about...the personal monitoring is higher than the individual location sampling.

Another point while we are on dust concentrations I might make - this is also in the same report from Health and Welfare - the figures will be in a thesis by Parrol, which has been submitted now for evaluation, on a mortality study in this

THE WITNESS: (cont'd.) manufacturing industry.

But I thought it might be interesting to just look at the sort of order of magnitude of the concentrations recorded on an impinger basis for different operations or different areas of the plant over time. Concentrations here, gain, they have dropped, but the highest values - the highest median value for a department is around just under ten. But concentrations did go up to over a hundred million particles in those plants.

Now, the problem we have in this particular population, this particular study, is that in insulation, in pipe manufacture and building products we are not working with pure fiber anymore and much of this dust that we see may be related to other silicates or to silica, or cement and so on, in the count.

So here if we were talking about a conversion of these data to something else, we would have probably an even bigger problem than we do in trying to convert within the mine, which is bad enough.

DR. DUPRE: Who is the author of the dissertation from which this table comes?

THE WITNESS: Well, this table is actually from... it's not from the dissertation. This table is from a report to the Department of National Health and Welfare, by Beckale, Gibbs and R. Harry - I think it's by Becklake and Gibbs with assistance from R. Harry. But Parrol, one of my graduate students undertook to do a master's degree on a mortality study, and as part of that study we worked him into assessment of exposure, for his purpose, and these are a summary of data that came out of that.

MR. LASKIN: Q. The published paper, the morbidity study published paper, I take it, is the first part of this study and what you are telling us about now is a mortality

Q. (cont'd.) study which followed with respect to one of the plants in Montreal?

A. Yes. There was...the original grants application was for a study of x-ray change and function changes in a manufacturing plant, one using chrysotile only, the other one using chrysotile plus amosite and crocidolite - a variety of other things. That's this particular plant.

Subsequently I had a graduate student who was interested in carrying out some research for his thesis and we decided that maybe a mortality study at this same plant would be worthwhile doing, so we integrated him into the study to do the mortality part, plus for his own study to work on the dust data for the assessment of exposure.

Q. I'm looking at tab fifteen of your compendium of articles, which appears to be the first part of the study.

A. Yes.

Q. Okay. Assuming we've got the right paper, looking at page 764 there appear to be two plants being compared with the mining study, and which...

A. That's the insulation and...it's this one, the insulation and cement products.

Q. Okay. So that plant A...well, the plant having insulation and cement products in tab fifteen is the same plant whose exposures are characterized by the slide that we've got in front of us now? Is that...

DR. DUPRE: Why does that table refer to plant B? "Summary Environmental Studies of Plant B".

THE WITNESS: In the report that we submitted to the Department of Health and Welfare, and in our communications with the plants, we had agreed with the plants that we would not identify the individual plants in the table. So we call it plant A and plant B in the report and submitted to the various

THE WITNESS: (cont'd.) plants the results of their surveys. So this report used A and B as a...and the subsequent paper may not have used the same nomenclature.

DR. DUPRE: Oh, I see. But it is the same plant as is here under A on page 764?

THE WITNESS: That's right.

DR. DUPRE: Okay.

MR. LASKIN: Q. I noted in page 764 it's described in terms of insulation and cement products, and yet when I look at the slide it appears that brake linings and textiles were also manufactured.

THE WITNESS: A. I think in the text you'll find that it does describe the other products, that were manufactured in that plant.

Q. And this is the plant with the mixed fiber exposure?

A. That's right. They had amosite, crocidolite and chrysotile in the particular plant, plus some silica.

DR. UFFEN: Before we leave that, you drew to our attention some of the important information in that chart. Am I right, and I see one piece of important information - "All locations

from 1957 to 1962, the median is three point nine

but the range goes from point one to a hundred

and twenty-one".

That's a factor of a thousand. That median is meaningless, isn't it?

THE WITNESS: The median is more meaningful than the mean, because the mean would be maybe a hundred, maybe twenty or something like that, depending on the number of values.

We have one value up here at a hundred and twenty-one. That might be the single value of one hundred and twenty-one that was ever measured. Therefore, any other median

THE WITNESS: (cont'd.) at least we know fifty percent of the values are below and fifty percent were above it.

DR. UFFEN: But the range being so broad, you need something besides just knowing the median. You have to have some other measure of the precision, eh?

THE WITNESS: Well, if one were to plot the data to show that they were log normal, and then if they were log normal you could put a geometric standard deviation on the values, and I'm not sure whether those particular values are log normal or not.

But the range...we've not hidden anything in the sense that we know what the range of those values are...

DR. UFFEN: Oh, I know it doesn't hide anything. It tells me a great deal. It tells me there is a spread of a thousandfold..

THE WITNESS: That's right, in the levels. That's right.

DR. UFFEN: Up to now I thought it was maybe about a factor of twenty.

THE WITNESS: No. It can be very, very large.

DR. UFFEN: Thank you.

THE WITNESS: For the same plant we had virtually no side-by-side midget impinger/membrane filter counts. They stopped using the midget impinger and went over to the membrane filter. So by 1972, they suddenly started with the membrane filter and here you can see again, perhaps having drawn attention to people, that we even have a bigger range of fiber concentrations for all locations in that area, and you can see that some of the fiber concentrations are quite...I mean, we are not talking one or two, we are talking very high concentrations.

Now, by 1974 to 1977, they had done major control in the plant and insulation, which was the major offender

THE WITNESS: (cont'd.) up in here, the asbestos insulation was phased out altogether in the plant, and now, of course, that figure disappears. But even in the other operations, in the pipe they are down to one point six from what was originally up to thirty-five. The concentration median has gone down from one point two to point four, so now we are at quite a different order of magnitude in recent years.

This is one of the problems you are faced with in trying to reproduce today what conversion factors might have been for the past, because if you go in here to measure membrane filter/midget impinger fiber ratios, you are dealing with a totally different order of magnitude than what you were ten, fifteen, twenty years ago.

MR. LASKIN: Q. Do you have on the previous slide the corresponding median figures for particles, that correspond to those time periods?

THE WITNESS: A. No, because there were no particle measurements made in those time periods, you see, so the...I'm pretty sure that's so...yes.

The figures that you saw at the bottom of the previous thing, which referred to Parrol, is that Parrol made an estimate or a guess at the 1972 to 1977 figures in terms of impinger, just to complete his data. But...

Q. They are not actual measurements?

A. They are not actual measurements, and that's why they are referred Parrol's. So there are no side-by-side figures.

I think at that point I would move into some other areas, and if that's okay we'll move from conversion into other questions.

Q. Sure.

A. In the early 1970's, and I guess back in the

A. (cont'd.) 1960's, the main question emphasis was being placed on the question of mesothelioma in relation to asbestos exposure, and much of the effort has been directed towards describing the etiological factors associated...etiological factors in the production of mesothelioma.

We also got involved in that in that we started work on fiber dimensions, looking at the dimensions of airborne fibers to which workers might be exposed. The important thing, I believe, to think about is that...and maybe a slide later I can draw attention to it again...is that we have seen a series of changes in scientific views about the etiological factors in asbestosis and in...not in asbestosis, but in lung cancer and in mesothelioma.

Starting with mesothelioma and lung cancer possibly being related to hydrocarbons associated with fibers, maybe trace metals associated with fibers, maybe fiber dimensions, in some cases people are thinking about surface properties of fibers and so on. We have to recognize we are dealing not only with one disease, but probably several diseases associated with asbestos. We have asbestosis, we have lung cancer, we have mesothelioma, we have possibly an increased risk of GI tract cancer, and we have no guarantees that the etiological factor in each of those causes is identical.

In fact, there is some evidence that maybe it's not. For example, if we look at mesothelioma, we've not been able to demonstrate any important link between smoking and asbestos exposure and increased risk of mesothelioma. Whereas for lung cancer, we have.

So in the one case we may have the possibility of synergistic affects of smoking, in the other we don't have that. So the mechanisms for lung cancer and other factors may be quite different.

THE WITNESS: (cont'd.) The same is true between fibrosis and maybe lung cancer, so the reason I'm saying that is as I go on to talk about dimensions of fibers and their importance in maybe health-related effects, it's important to think that there may be other etiological factors acting in addition to size and shape, in different situations, and we may have many more than one single etiology.

What started us looking, perhaps, at this question? If one looks at the...this is taken from the McDonald material on...maybe I should give you the headings first: This is the McDonald and McDonald paper on the Epidemiology of Mesothelioma, where they compiled the mesothelioma existing up until, I think the paper is published in 1977 or 1978, thereabouts.

Now, what we were interested in is whether or not there were any differences in the rates of mesothelioma or lung cancer in different occupational groups.

Now ideally one would like to have the same level of exposure, using the same parameter to measure the exposure, in different occupational groups. That does not exist.

So what we did was to take a look at what differences there might be in mesothelioma rates. Here you can see the ratio of mesothelioma to all deaths, the ratio of lung cancer to all deaths, the pleural mesothelioma percent, and the number of deaths and the number of people in the various studies.

You can see that in insulation workers the proportion of mortality was running around seven to eight percent, in quite a range of people working with insulation material.

In the asbestos textile factory in Rochester,

THE WITNESS: (cont'd.) it was around three point two percent. These are mixed products.

In London, mixed products, seven percent.

Pennsylvania, two point five percent.

A low figure here in a Cardiff mixed-product plant.

In the Paterson, New Jersey, around one point four percent.

Mining chrysotile, Quebec, point one nine percent.

In Italy, no mesotheliomas reported in the population.

In Finland, no mesotheliomas reported in the population.

In talc, there were some mesotheliomas, again with two point two. The talc including tremolite, tremolitic talc.

What we were looking for was to see if there was any evidence of differences in the rates of mesotheliomas in different occupational groups which might bear explanation both in exposure level and in fiber type, or in changes of dimension of fibers.

Subsequent, of course, to this work, there has been the gas mask study by the two McDonalds, which showed a very high rate of mesothelioma...

DR. DUPRE: Excuse me, Dr. Gibbs, before you show that table.

THE WITNESS: Sorry?

DR. DUPRE: That last table came from McDonald and McDonald, which for our reference purposes, counsel, would be what exhibit we've had?

MR. LASKIN: Tab twelve of exhibit eighteen, table twelve in tab twelve, the McDonald compendium.

DR. DUPRE: Thank you.

THE WITNESS: Okay. More recently there has been the study, both the study in the U.K. and in Canada. This study by the McDonalds again, and the previous study by Jones et al on gas mask workers, and here we see that the rate of mesothelioma in the population, sixteen percent in the gas mask workers, and point two six percent in the chrysotile miners and millers. Again, a big difference in the occurrence of mesothelioma.

In fact, in this case also we see almost a doubling of the lung cancer proportion in the population.

What in essence is the etiology...what are the etiological factors in mesothelioma?

Well, these studies, the epidemiological studies, had suggested that where certain types of crocidolite had been used there was an increased risk of mesothelioma. If we...South Africa, the Cape crocidolite mining industry, had a fair number of mesotheliomas occurring.

A more recent paper by Hobbs from Australia, who started studying Australian crocidolite workers, again shows a fair number of mesotheliomas occurring in that group.

Here, in the gas mask workers where Australian crocidolite was used, again a big difference. The only exception, perhaps, to the crocidolite story appears to be in the lack of mesotheliomas reported from the Transvaal crocidolite mining area.

Now, what was being done on a world scale...not on a world scale, but in different countries, to try and look at the etiology of mesothelioma?

Well, one approach was to look at the occurrence of fibers in tissue, and Pooley in Cardiff carried out a series with Alison McDonald of analyses in tissue, and all I would like to show you is just one table which is from a report to the

THE WITNESS: (cont'd.) Department of National Health and Welfare. It's not part of...I don't think it's part of the compendium of papers that you have. It's fiber type and mesothelioma, and these analyses were carried out actually by Dr. Rowlands of tissues from case control pairs in Canada, and what we were ...I don't know whether I can make that smaller... if we look at the cases, and it's a small number of cases that were in this series, I think it's a total of twenty cases...we look on the lefthand side where the cases occur, and it's a small number. We find amphibole in three cases, crocidolite occurring in fairly high concentrations per numbers of fibers per grid, in one case;- amosite, a high proportion in another; tremolite, a high proportion in another.

On the other...in the control side, we find chrysotile in the presence of our control population, and either no fiber types present or chrysotile present in fairly high percentage in the control population.

The reason for showing it is that this is a very small study. One wouldn't reach any real conclusions from it. Nevertheless, the pattern is almost identical to that which has been found by Pooley, that in the control population you can get chrysotile fibers present in the lungs. Sometimes all the fibers present might be that, but in the cases, the tendency is very much to have more amphibole present in there.

Now, that raises the whole question of is this due to fibers being removed from the lung, or is it, because you are always looking at autopsy material, looking at what is residual, not necessarily what was originally deposited there.

At this point in time, I don't think we have a straight answer to that. There was, will be a publication in the British Occupational...in the proceedings of the British Occupational Hygiene Society meeting. At this point that paper

THE WITNESS: (cont'd.) is not published, it's in press.

The material was presented by Dr. Rowlands in Cardiff last year, in which in the Quebec chrysotile miners we attempted to look at whether or not chrysotile fiber was present in higher concentrations in the lungs of workers who had died a short time after last exposure, compared with workers who died a long time after exposure, taking account of their level of exposure during their lifetime.

Now, that part of the study I'm not sure that we will get an answer to that question. Perhaps Dr. Rowlands might be asked about that at some other time.

What is interesting is that in that study we did find in the lungs of our chrysotile miners, tremolite fibers, very, very commonly. In fact we found more tremolite in many instances, than we found chrysotile.

Now again, that isn't perhaps new in that Pooley had previously examined some of the lungs from the chrysotile miners, asbestosis cases, and had found tremolite in the lung and previously reported that.

What we have done is subsequently confirmed that indeed there is a fair amount of tremolite fiber present in the lungs of the Quebec chrysotile miners, and this has implications for the type of study on type of fiber in lung tissue for mesothelioma cases, because we still don't know whether it's because the chrysotile didn't get there in the first place or whether it got removed. But there's no doubt that the amphibole fibers are still turning up quite clearly in the lung, and the amphiboles' fibers are certainly found very clearly in the lungs in mesothelioma, many of the mesothelioma cases studied in the case control mesothelioma studies done in the U.K. and in our Canadian one.

THE WITNESS: (cont'd.) Can I maybe go to the next...this is just perhaps a quick recap of some of the things I've said.

Measurements have been made in tissue to characterize the type of exposure. Some attempt has been made to identify fiber dimensions and penetration, to link fiber type to health effect and to determine the order of magnitude of exposure.

Now I would caution, in terms of determination of order of magnitude of exposure, our figures would suggest - even in instances where we know we've had a very high level of chrysotile exposure - in the lung tissue we have not found a lot of chrysotile fibers. We have found some tremolite fibers. And so the implications about using lung tissue for certain fiber types may not be as good as we would like them to be for assessing past exposure to fiber. We are looking at retention in those situations.

UNIDENTIFIED SPEAKER: May I make a comment now, Mr. Chairman, just to qualify that statement slightly?

I think the most important fact is we have found a lot more tremolite than would be expected. Although we might have found high exposures of chrysotile, we have found equally high exposures in the same autopsy sample of, say tremolite.

So it's the difference between the two, the ratio between the two that...

THE WITNESS: Okay. I didn't want to infer that we don't find chrysotile in the lung tissues, and thank you for correcting me. We do find chrysotile in the lung tissues, but we also find very high amounts of tremolite in those tissues. The question of whether or not we can use those figures to assess previous exposure is still open to question.

DR. UFFEN: Could we have a little diversion here

DR. UFFEN: (cont'd.) just for a minute?

Could you offer any comment as to where is the tremolite coming from? Does it come from the same ore that the chrysotile comes from, or is it coming from the host rock? The serpentine?

THE WITNESS: I think...I don't have an answer to that. We've looked and discussed where could the tremolite come from. There have been several suggestions.

There certainly is a fair amount of altered serpentine and intruded serpentine with some talc, and possibly some tremolite, in it in the mines. One suggestion has been that maybe workers worked at one point through parts of the ore body which contained a fair amount of tremolite and we are seeing the results of that today.

If you look at the gross, the total fiber, you have a job to find, if ever, any tremolite in the fiber. So in the bulk fiber today it doesn't seem to be a major component.

The other possibility though might be that even though you start with a very small amount of tremolite, if the lung were retaining that fiber - the lung has a very effective mechanism for filtering out and removing certain sizes and dimensions of particles, and it is possible that that's what it does - it concentrates certain types of fiber.

One other suggestion which may be equally plausible is that I understand that at some points children in the Eastern Townships used to do a fair amount of carving of soapstone and it was suggested to me that maybe they were working with tremolitic talcs in their homes and they got exposure to the material, and what we are seeing is a result of that.

The quick answer is, I don't know. I can only hypothesize at this point.

MISS KAHN: Next one?

THE WITNESS: Yes, please.

I think...go to the next, please...I wanted to mention that something which has come up on a number of occasions, and in the previous one which I just shot through there, the tissue preparation stages. One of the problems in doing fiber analyses in tissue is how well characterized are the tissues that you are working with? Where did they come from?

Very often, and it's certainly true of the series I showed you earlier, we had great difficulty in finding out where the pathologist actually took the specimen from.

If it was meant to be from the parenchyma, it was often very poorly identified.

Secondly, the way in which samples are stored. To give you an example, on one occasion we were asked by a compensation board to look at a lung tissue for asbestos fibers, and when we looked at the tissue, the tissue was loaded with needle-like looking material and we thought, gee, the fellow must have been absolutely grossly exposed to some fibrous material.

We then did an analysis on the fiber type and all we found was potassium. And we couldn't understand that, so we made some inquiries. It turned out that the tissues had been stored in a potassium oxylate solution, and when the tissues were removed and dry, the fibers had crystallized very nicely into needle-like form and our technique for preparation had not destroyed those, and the tissue was absolutely loaded with potassium oxylate needles, so one could be easily misled by just looking down the microscope or doing electron microscopy alone on the tissue, if one doesn't know enough about how it's stored and the storing can interfere with what you are looking at.

The question of magnesium leaching has come up in terms of tissue storing, and opinions will vary whether magnesium leaching occurs in tissue or in the storing of tissues

THE WITNESS: (cont'd.) in formalin, for example.

I think from our own laboratory's standpoint I'm not sure whether we can answer that question one way or another. It's possible that, again, at a later date Dr. Rowlands, who has looked at some of these things recently, would be here to comment on that.

But certainly from discussions from international meetings with people on this question, we have seen some fibers which appear to be magnesium deficient, in some tissues. Other people have seen some fibers of that nature, and it has become a bone of contention in terms of what you call a fiber, because if a fiber is there and it doesn't have a lot of magnesium in it, if you say it was magnesium leached, therefore I'm going to call it chrysotile, you may or may not be right.

So one has to think about what are the boundaries that you put on the magnesium leaching content before you would classify it that way.

Fiber coatings are a problem as well, with ferruginous bodies, coatings which give you a high iron analysis, and so on.

Can I have the next slide, please?

Can we go to the next one? I think we discussed this just now, the question of...sorry, can we go...this is what I showed you on the slides just now. We could move on.

These are the proportional mortality for mesothelioma in the different studies, and this does include the gas mask workers and it's worth...could we just move it over slightly?

MISS KAHN: Pardon?

THE WITNESS: Can we move the slide over slightly? That's right.

It is interesting here, you can see quite clearly

THE WITNESS:(cont'd.) the difference between the different occupational groups, ranging from no mesotheliomas in the Italian and the anthophyllite, right up to the sixteen percent in the gas mask workers, with the mixed fiber type and the insulation workers falling somewhere in between...and the amosite factory perhaps closer, if anything, although still substantially greater than the chrysotile.

The next slide, please.

Okay. The importance of fibers in the production of mesothelioma, and of other biological reaction, depend essentially on their ability to enter the respiratory system, where they are deposited within the respiratory system, the extent to which they may be retained at the deposition site, the rates of removel of the fibers, or their translocation to other locations, and the biological activity of the fibers or their ability to carry with them some other biologically-active material that might give rise to the effect.

Can I have the next slide, please?

What we know about the aerodynamic behaviour of fibers comes from work done by Timbrell, which shows us that for a fiber settling in air,the rate at which it settles depends on its actual diameter. If we consider entry of fibers into the respiratory system, two or three parameters come into play.

First, the diameter, because one of the mechanisms for deposition in the human respiratory system is diameter.

Secondly, length, because as fibers try to pass through airways which are fairly narrow, a fiber which is fairly long and gets out of orientation so it's angled to the wall will be trapped. Also, if there is a change in direction of air flow, as there would be at a branching, there could be momentum of the fiber which would carry the fiber on to the branch.

THE WITNESS: (cont'd.) So in terms of the importance of fiber size for entry into the body, there is pretty good physical evidence or physical knowledge, physical science knowledge, on which to make some assumptions about entry of fibers into the respiratory system.

What Timbrell did, he took some airborne fibers and some spherical particles and he compared the rates, settling rate, of these spheres and these fibers.

What he was able to show is that for asbestos fibers, because settling rate depends on density and diameter, for asbestos fibers, fibers greater than around three to three and a half microns would not penetrate directly into the alveolar region.

He also was able to show that interception would occur, which could reduce the quantity of fibers entering the lung in experimental animals.

He also was able to show, with the UICC sample, that he could get a much lower deposition of chrysotile in the lung than the other fibers, the straight amphibole fibers, glass fibers and crocidolite fibers.

MR. LASKIN: Q. What's the explanation for that?

THE WITNESS: A. The argument he made was that the curliness of the fiber led to a greater cross-sectional area as the fiber entered, and hence interception at branches and in nasal passages would be much greater. Therefore, chrysotile fibers would not get down deep into the lungs.

I'll come back to touch on that later, because in practice if we take air samples in industry, we did not find the nice curly fibers that Timbrell has demonstrated for the UICC samples. The actual world appears to be quite different from what the UICC samples show.

What...sorry, could we leave that one in?

MISS KAHN: Same one?

THE WITNESS: Yes, please.

By 1974, there really were no data available, believe it or not, concerning the dimensions of airborne fibers to which people were exposed.

The exception being the work that Timbrell had done using aerosol spectrometer, and with a few samples of dust collected from the Transvaal and the Cape Province area in South Africa.

The information on the normal size distribution of airborne fibers in industry was very seriously lacking.

So we began a study to try and determine in air the actual dimensions of airborne fibers to which workers were exposed.

Now, to do that our first attempt was with scanning electron microscopy, and that has been published. We subsequently have some reservations about that work, reservations not about the results that we got with the technique, but some limitations of the technique which I think are worth mentioning.

First of all, in order to use scanning electron microscopy we have to collect the airborne sample on a flat surface in some way, because the way in which the microscope works, it scans across the surface and finds the bumps where the fibers are.

Secondly, because the filters, the ones which are normally used and the ones we used were polycarbonate filters, nucleipore, the nucleipore filters get highly charged and are very difficult to handle. Secondly, thirdly, they have pores in them of a particular size which can be from a micron, maybe greater, down to point one micron. At point one micron they have a very high resistance, so they are very difficult for sampling. As we go to larger-sized pores in them, we run the risk that fibers

THE WITNESS: (cont'd.) which get orientated with the filter will pass straight through them.

We also have the difficulty with nucleipore, with the scanning EM technique, that we may get charging on the filter in the electron microscope, so we have to coat the fiber.

Now, the effect of coating the fiber is that we may lose to observation some of the very narrow fibers, some of the hundred micron fibers might get lost. Some fibers may pass through the filter, and it has been our experience that if we look at scanning EM distributions, which we did originally and then we did some further followup with scanning EM, on the SEM we tend to see more longer fibers, but a serious deficit of the short, narrow fibers on the filter.

For that reason we decided to adopt a transmission electron microscopic technique to look at the airborne fiber distribution and to followup on the slight modification of the U.S. technique for transferring fibers to grids, to retain the fibers exactly, as far as possible, exactly as they existed in air originally. Because we want to know what are the dimension of these fibers as they exist in the breathing zone of the worker, or in the air in which the worker works, without disruption.

Now here, you can get some idea of what fibers might look like in different operations. I would be erroneous to say these are representative. Always one picks some of the better examples to show. But they indicate that in the mine we might have clumps, fairly large clumps of fiber. By the time we get through the bagging area, fibers now as part of the process, we have made attempts to try and break them up and to make them into more narrow fibers.

Could I have the next slide, please?

Here, for crocidolite, you can see examples in

THE WITNESS: (cont'd.) the pit, and in bagging. Again, there are slightly more narrower fibers breaking off from the crocidolite perhaps in the second one than in the first one.

Next slide, please?

If we look at crocidolite and chrysotile in a pit operation, you can see that we have some...I don't know whether it's a very good example, but with chrysotile we tend to have quite a few of these small pieces around with bent fibers attached to clumps, and crocidolite, we've got more straightforward bundles of fibers.

Can I have the next slide, please?

Give me the next slide, please. I think that's not a very good...

MR. LASKIN: Q. You may be coming to it and I don't want to take you away from it, but are you going to link up the apparent differing results that you demonstrated for mesothelioma with ...

THE WITNESS: A. That's where eventually...yes, that's where I'm heading. We'll look at the slides in a moment. We'll come to a few.

But I wanted to give you some idea of what these airborne fibers look like, and here is an example of three different types of asbestos in the same process for the righthand side, except on the lefthand side there is carding with chrysotile.

You can see that the amosite fiber is a relatively fat fiber, and these are the same magnification.

The righthand side, the amosite is a fairly fat fiber, the crocidolite is a narrow, straight fiber, and the chrysotile fiber can, when it reaches length, be quite curved.

Can I have the next?

Can we have the next one?

THE WITNESS: (cont'd.) Sorry. I had envisaged I could press the button and flip by them quickly, but when you are putting them in it's a very tedious...

Okay. What we wanted to do was to measure...we could measure a number of parameters for a fiber, and as I mentioned, this has really not been, except for Timbrell's work, has not been attempted in a systematic way for work environments before.

We could measure what we called TD, the true diameter. We could measure the true length of the fiber. We could measure the coil diameter when a fiber...as you see on the lefthand side, the bottom...when a fiber is somewhat curled, it has a greater cross-sectional area, so we measured its coil diameter which would give us some idea of its curliness. We would also measure its coil length, which would be its total length of the coil.

Now in some instances particles would adhere to the particle and it would be possible for the coil length to be somewhat more than the true length. That presented a little bit of a problem.

But these are the parameters we decided to measure.

Can I have the next slide?

Okay. Could we....now, this material reflects the sizes of fibers as measured by transmission electron microscopy.

Now, I must mention...perhaps I think I've got a slide later that will show this...no, let's look at this one first.

This shows a comparison of the dimensions of fibers collected in chrysotile mining, amosite mining, crocidolite mining operations. It gives you some idea of the true diameter differences. If you look at the median true diameter for amosite, it is substantially greater than the median true diameter for chrysotile or crocidolite.

THE WITNESS: (cont'd.) If we look at the true length of fibers, we find that the amosite fibers were substantially longer, the median was longer, than the crocidolite or chrysotile, and also that the crocidolite is somewhat longer than the chrysotile in the mining operation.

I won't go into any detail on the coil diameter and so on. You can see the coil diameters are somewhat greater than the true diameters. This is because there is, even in the case of crocidilite and amosite, some attachment of other particles or other fibers which make them appear to be slightly bent, and this is how they get measured.

The other thing I would like to draw attention to is the question of the aspect ratio. You can see that crocidolite fibers tend to be, have a higher aspect ratio than amosite and chrysotile. Now, this is because they have a fairly narrow diameter and are a fair length, whereas amosite on the other hand has a very fat diameter and is very long.

It's this fact that amosite has a slightly fatter diameter that reduces its aspect ratio, and chrysotile is the lowest aspect ratio - length-to-breadth ratio.

On a mass basis, this is also very important, you can see that the mass of amosite is substantially greater... the mass of a single fiber, the average mass of an airborne fiber of amosite...is considerably greater than the mass of crocidolite, which in turn is also considerably greater than the mass of a chrysotile fiber in airborne dust.

This has quite important implications in terms of standards, because what it means, if the average median mass is that much different, if you took the same concentration on a mass basis, the number of fibers to which people would be exposed would be substantially different for the different fiber types.

THE WITNESS: (cont'd.) If, on the other hand, you set your standard on a fiber basis, by the number basis, the mass of dust to which different workers would be exposed would also be substantially different.

So you've got to know in advance whether or not your health effect is indeed due to mass or is it due to number, because if it's due to mass, then your setting of a number standard would put the weight in the wrong place for the protection of the employee.

MR. LASKIN: Q. Is there any evidence on that issue?

DR. UFFEN: No pun implied?

THE WITNESS: Pardon?

DR. UFFEN: No pun implied?

THE WITNESS: No pun implied.

MR. LASKIN: Q. Is there any evidence on that issue?

THE WITNESS: On the question of whether mass or number is important?

Q. We've heard a lot about number, but is there anything on the question of mass?

A. There was some work...I can't point you immediately to it...but there was some experimental work, I think, done in Cardiff by Skidmore, where they used a uniform mass concentration of the different fiber types to see what happened, in an attempt to see whether, I think it was fibrosis, might be related to mass or number.

DR. DUPRE: I'm just intrigued at this point, Dr. Gibbs, in the fact that apparently the German regulatory regime, perhaps with characteristic confidence, appears to have both a mass concentration standard and a fiber concentration standard, thereby perhaps hedging bets on this point.

THE WITNESS: Yes.

DR. DUPRE: Is there something to recommend a consideration of that kind of dual standard? I understand that the German one is one milligram per cubic meter, it's being revised downward to point zero five milligrams, and then there is also a fiber concentration standard of two fibers per cubic centimeter, coming down to one.

THE WITNESS: In fact, Quebec also has the same thing. It also has a mass and a number standard.

It has several...I think it has five standards altogether, if I remember...it has a number standard, it has a total respirable mass standard, and it has, I think it's a ... it has a recirculating air standard based on a respirable mass but with a limit on the proportion of asbestos in that respirable mass.

I don't think when they were originally set that they were set with the intention of taking account of both, but I don't think that's how...the reason for them.

DR. DUPRE: I don't pretend to any expertise on the German front for a moment, but my initial reading led me, perhaps erroneously, to assume that they are using both standards simultaneously.

THE WITNESS: Yes. They may well be using those simultaneously, but I don't think their logic originally was because of possible differences between fiber types. I could be wrong.

MR. LASKIN: Q. Just looking at a comment in one of your own articles, which is at tab nine on page 464, where you, yourself, seem to suggest that perhaps there is some relationship between asbestosis and mass, rather than number. Looking at the righthand side of the column, the first full paragraph.

THE WITNESS: A. That's right. I think you'll see the paper I refer to, nineteen. It's not a paper, that's a Skidmore personal communication, but it's work being carried out by Skidmore in the U.K.

I think what I had argued here might be occurring was that in the studies that we knew of at that point in time, the Quebec, the prevalence of radiological changes in the Quebec asbestos miners were substantially less than in the insulation workers reported by Selikoff and also reported by Harries in the U.K.

The problem, serious problem, in making comparisons of radiological changes is that we know there are large differences between different readers that leads to wide variation. We talk about dust variation, but there's an interobserver variation in x-ray reading, and one is not quite sure how valid the statement might be now that there would be a lower rate in the chrysotile miners than in the insulation workers, except in this particular case Harries had worked with Rossiter, who had produced a Quebec chrysotile paper, and some of the readers that were in the Quebec study had also read some x-rays in that study, so Rossiter had looked to see how comparable the reading level was in the two groups.

So people with similar periods of exposure in the two groups, we were getting similar, quite different rates of radiological change, and we also had some good evidence that chrysotile miners were probably more exposed than the insulation workers.

So this was hypothesis that possibly that was an explanation. Combine that with the animal data and maybe there is something in there, but I don't think it's substantiated, we don't know what is the best indicator index to predict asbestosis or to predict cancer. All we know is certain indices do

THE WITNESS: (cont'd.) predict.

Can I have the next?

This is to show you the same data, but now for the bagging operation, and you can still see that amosite fibers are fatter than the other types of fibers. The true length of the amosite fibers, again it's longer, and you can see the mass in the bagging area is also substantially greater for the amosite fibers than for the crocidolite fibers, with some changes in...slight changes...in the median mass now of the chrysotile and the crocidolite, with change in process.

DR. UFFEN: My memory is failing me. I don't remember how the mass is obtained. Is it by measuring the number of particles by a density, or..?

THE WITNESS: The mass of the individual fibers was obtained by calculating for each fiber measured, based on its diameter and its length, and assuming spherocity, which is not strictly correct for the crocidolite is slightly elliptical in cross-section, assuming spherocity and assuming that the bulk density of crocidolite and the other fibers is the same as the airborne density, which also may or may not be absolutely the same, we've calculated the mass of each individual fiber and then looked at the median mass of the...

DR. UFFEN: Did somebody measure the bulk density of crocidolite and chrysotile and amosite?

THE WITNESS: We took it from the literature.

DR. UFFEN: It was taken from the literature?

THE WITNESS: The density of crocidolite, amosite and chrysotile.

DR. UFFEN: You took the density from an accepted measurement, but you don't know where, how or anything else...at the moment anyway?

THE WITNESS: Well, the density for chrysotile,

THE WITNESS: (cont'd.) crocidolite and amosite have been well reported, and the values that had been obtained for it had been very similar. Our concern in terms of applying that density to an airborne fiber is that we are dealing usually in the measurement of the density with bulk material, and here we are talking about fibers which are very, very small, in which other particles will not be a factor in the density determination.

DR. UFFEN: I understand the difficulty there, but it's only just dawned on me that the original density for the different types of fibers ...

THE WITNESS: Are different.

DR. UFFEN:...may be doubtful, or may not. I mean, do they differ very much?

THE WITNESS: They don't differ a lot, but there are differences between the densities of amosite, crocidolite and chrysotile, chrysotile being the less dense and amosite being the more dense. The densities, though, have been published many, many times. There seems to be fairly good data on the density of the amphiboles and the chrysotile fiber, bulk density.

DR. UFFEN: How would they have got those both densities in the first place? By measuring fibers and volumes? Then we've gone around nicely in a circle.

How do you measure the density of a fiber? I've never...

THE WITNESS: An individual fiber?

DR. UFFEN: Yes.

THE WITNESS: Well, I don't think that the density of individual fibers has ever been measured, which is our problem. Because we are dealing with fibers which will be very difficult to manipulate to determine individual fibers.

If we were to do them through, one approach might be displacement procedures as would be used for bulk mineral

THE WITNESS: (cont'd.) determination where you might use a bulk displacement approach.

That has a problem for us in that we don't know how much of the density is related to other materials adhering to the fiber. Nevertheless, I've not seen in the literature much argument over the measured density values that are used for industrial purposes.

DR. UFFEN: There might not be much argument over it for industrial purposes. Then when you start to talk about its biological effect and so on, it would be a little disconcerting if you discovered that you took a bulk sample of fibrous material, measured its mass, divided by some number to get the mass per fiber, thus getting an average density, and then proceeded to use that to recalculate the mass of little particles under completely different circumstances.

It would seem to me the chances of having enormous errors in these masses would be considerable. I would be glad if somebody could relieve me of this concern.

UNIDENTIFIED SPEAKER: That's the smallest of the errors. If you are fifty percent out on your SG determinations, it would be the smallest of your errors compared to all the other errors you could make in your measurements.

DR. UFFEN: But it is one more that we haven't had before us.

THE WITNESS: I'm sure, yes. The list could go on and on.

UNIDENTIFIED SPEAKER: If your SG was one point five instead of two point five, the variation - if you are looking at it on a mass basis - would be far greater.

THE WITNESS: I would think that you are not going to be far out in your density. I mean, your density which is crocidolite is three and...three point five, I think

THE WITNESS: (cont'd.) it is...if you used another technique to get at it...and I doubt if it has been done that route - it certainly wouldn't have been done by the electron microscope route to calculate density, of counting, and it's unlikely that it was going to be six in error. It might be three point seven, or it might be three point two, but I don't think you would have a large variation in the density.

There is a problem, though, with aerodynamic densities in that if therewere, say holes in the middles of fibers, in the case of chrysotile, for example, there is a Swiss roll thing, what is the effect of the hollow core on the actual, what should be the density that we use for calculated the aerodynamic behaviour? I don't know.

The assumption has been made by other researchers that the bulk density is indeed a valid figure to use.

Could I maybe...could we go to the next, please?

Okay. I mentioned earlier that one of the problems we had with scanning electron microscopy was that we lost very short, narrow fibers. On the other hand, if we use transmission electron microscopy we have the problem that our fibers are mounted on a grid to put into the electron microscope.

Now, as the transmission electron microscope looks through the sample, the edges of the grid bar interfere with sometimes fibers which might overlap that grid bar, and in our study the maximum fiber length that we could see would have been somewhere of the order of fifty microns or so, because of this problem of overlap with the grid bar.

Now, the fact that our grid area was such also meant the probability of seeing fibers as their length increased - and it's true with any transmission electron microscopic technique - as the fiber sizes increase, your probability of

THE WITNESS: (cont'd.) some part of the fiber intercepting the edge of the field increases, and hence your chances of accurately reflecting that fiber decrease.

So in order to complete the picture in terms of length, we took from the same airborne sample a segment which we did measurements using optical microscopy. Here you can see the total length distribution for each of the fiber types, for mining and for bagging.

Now, what you see is a slight decrease in all cases, perhaps, in the proportion of fibers in the less than one micron size range, and in the overall proportion of fibers greater than between five and a hundred microns, a slight increase from mining through to the bagging area.

With crocidolite it seems to have gone from four to seven, in mining from twelve to twenty-four; and in chrysotile, from one point two to four, an increase in the length of the fibers that one is seeing present in the atmosphere for the later stages of bagging, when one looks at the total length distribution. Because I showed you earlier - which is why median is perhaps such a useful figure to look at - we did not have complete information, obviously, on the long fibers, because the transmission EM sees the short fibers very well, it doesn't see the long fibers. As a very high proportion of all fibers are less than one micron in length, very narrow, then we see most of the fibers by transmission EM, but not all of them...it's short on long fibers.

The next, please.

Now, this is just to give you some example, some idea, of what the size distributions look like. This is length distribution of fibers as observed by the transmission EM, and also by the optical microscope.

If you look in the mining at the top, the bagging

THE WITNESS: (cont'd.) area in the bottom, you can see that what we measure on the optical microscope only overlaps very slightly with what we observed on the electron microscope. That little intervening period where we are measuring both on the EM and on the optical microscope, and the EM loses those long fiber and the optical microscope tends to lose the short fibers.

Can I have the next one, please?

Now, if we look at diameter, what we see is this - we had no fibers in our airborne dust cloud that were greater than three microns in diameter. That's the limit that Timbrell claimed determines whether fiber can enter deeply into the alveolar region of the lung.

So in essence what we are saying is that one hundred percent of the fibers we saw present in the airborne dust in both, in each of these mining operations, was potentially respirable, could penetrate into the alveolar region.

The interesting thing where we look at the diameter distribution is the strange behaviour of amosite. It certainly is not unimodel. It's multimodel in some way, and it is quite different in its size distribution from the other varieties.

You can also see that the chrysotile fibers tend to be narrower in diameter than the crocidolite, and in turn somewhat narrower than the amosite fibers.

Can I have the next slide, please?

Now, one thing which has not been attempted prior to the first publication by us was looking at the diameter and length matrix. Now, later Stanton used similar data, used a similar approach, in classifying his fibers for his study of implantation of glass fibers on the pleurae, of fiber glass, to produce mesothelioma.

What we attempted to do was to see whether or not there were any major changes, major differences, between

THE WITNESS: (cont'd.) asbestos fibers of different types or different processes, which might pinpoint a particular cell or groups of cells which one might be able to say the epidemiology suggests that crocidolite exposure seems to be more, produce more mesotheliomas than does chrysotile and amosite. Is there any cell or group of cells of sizes which might fit with what appears to be happening from the epidemiological standpoint.

Then also to look at the experimental data and say, does this hold up if we use the experiment data and apply it to this, would this predict what is happening in man?

We haven't got quite to that stage yet. What I can tell you is happening, the dimensions of amosite, crocidolite and chrysotile are distinctly different. We can distinguish very well between the three major types.

When we start looking at whether or not there are changes between different operations, from the mining through bagging into manufacture, for example, there are some changes in the dimensions of the fibers, but it is much more difficult to demonstrate clearly that these are different.

That obviously becomes very important in trying to explain, for example, the Dement study we discussed earlier. One may argue have fibers become finer in that part of the industry, and hence is that an explanation for why, if all the dose data were okay, comparable to other studies, was it some other dimension effect.

I don't know.

For crocidolite, the dimensions do not change very much with processing. In other words, in contrast to perhaps chrysotile which changes a bit more, the crocidolite fiber in mining is very similar to what it is in bagging, which in turn is very similar to what you find in the manufacturing operation.

Maybe we could have the next slide?

THE WITNESS: (contd.) You can see here, for example, if we look at the...this would be length or...could I see the bottom? Diameter. If we look at the diameter of crocidolite fibers in mining or storage, crushing, bagging and so on, they are all clumped around together.

The one which is different, number seven, is a cutting operation in a pipe manufacturing plant in South Africa. There, chrysotile fibers are also present because chrysotile was mixed with the crocidolite in the pipe manufacture, and immediately the diameter distribution has been shifted right back.

But crocidolite diameters fall within that little package of diameter.

The next slide, please.

The same is somewhat true of true length, though we don't show the very long end of the spectrum here, and there are some differences, perhaps, at the long end of the spectrum, but again, for different locations, different operations, the length distribution of the crocidolite fibers is not that different.

But when we introduce chrysotile, the distribution again changes quite markedly.

The next slide, please.

The question...

MR. LASKIN: Q. What's the situation with amosite, just before you go on?

THE WITNESS: A. In terms of its change in dimension?

We have the data on the amosite and we have a...there is a graduate student who is just completing his thesis work on the differences between amosite, chrysotile and crocidolite, in their distributions.

THE WITNESS: (cont'd.) Assuming that he passes his exam, the thesis should be available by the early fall on that work. Although I have read his thesis on a number of occasions, there is a tremendous volume of data available in that, and I think I would like to look at that in more detail if I were to comment on what one might extract from that.

But he has looked at fairly large numbers of amosite, chrysotile and crocidolite fibers, at what happens to them from mining through milling, and...I'm sorry?

Q. I wasn't clear. Do you now have a judgement on whether chrysotile changes through these various processes?

A. If I...I would be quoting from his data, I think. I will show you...I have got something or I can give you some figures on that question, if I can find them.

MR. HARDY: Counsel, while we are talking about this, are we going to get copies of these slides we have just been looking at, which I don't believe are in the...

MR. LASKIN: No, they are not, and I hope with Dr. Gibbs' co-operation we will be able to do that.

THE WITNESS: A number of them are in the Lyon paper, which is in here. The others...but that's no problem. We can arrange for you to have copies...

MR. LASKIN: I don't want to take you away from what you were doing, so perhaps when we take a break you might be able to find those figures.

Do you know the answer to the question as to whether these kinds of size distributions that you have been talking about in respect of chrysotile have changed over the years? Instead of looking at change in diameter or length as one goes from mining through various processes, have you looked at the question as to whether there is a difference in length or diameter in mining twenty years ago as opposed to mining today, or textiles ten years

Q. (cont'd.) ago as opposed to textiles today?

A. I think it's a very good question, and in the... answer is no, we have not. One of the assumptions, one of the difficulties perhaps, in reaching conclusions about the past exposure of workers, is that in this area of dimensional changes it is quite possible that a number of factors have played an important role.

If we take the crocidolite mining industry in South Africa, which we have published a paper on the dimensions in the Koorman area, we very specifically stated the Koorman area and I believe we mentioned problems of not being able to extrapolate back to what conditions were before.

Now, I recall quite clearly in the...in 1960, when I worked with MRC, we received some amosite fibers from South Africa, and we received two types - some which were very, very brown, and some which were very, very white. They came from the weathered and the unweathered zone in the mining zone, and as mining has progressed, of course some of the weathered zone fiber disappears, and so the proportion...there is a visible characteristic which was an iron oxidation problem in the fiber, but there was a visible difference difference between the fiber at that point in time, which we know has changed from now.

The level of mining when asbestos was first produced was at the surface pretty well. Now we are fairly deep in some areas.

Q. If I can just focus in for a moment specifically on Quebec, the reason I asked the question is that when Dr. Acheson was here and testified, he drew our attention in particular to a footnote which appeared in his report and which I think may still be in front of you. I think it's at page thirty-four. I think you've got it right on top.

You might just want to have a quick look at that

Q. (cont'd.) footnote for a moment.

As I recall his evidence, and the transcript will certainly correct me if I'm wrong, but as I recall his evidence his view was that the Quebec Asbestos Mining Association would have the kind of data that would tell us the answer to this question, and he indicated that while he requested the data, he didn't get it.

I'm just wondering in view of the work which you did in Quebec whether you can help us at all as to whether that data is available, and if so, do you have any idea whether it demonstrates or supports what is indicated in that footnote, or not?

A. I have never requested from them information on the changes in the surface characteristics of the fibers from the mines...the surface area changes in the fiber from the mines. I do have somewhere some data on the grade production from the mines for different years, which would be a measure of whether they were producing more long or short fiber, depending on the proportion of three to seven. I don't know whether the data would exist on surface area measurements on a routine basis for different years.

I'm not quite sure which year the Quebec asbestos mining industry introduced some of its test procedures which became, now of course are world accepted as test procedures for asbestos. I'm not quite sure what year they would have been introduced and whether or not there would be information available on surface area measurements, which is probably what he is referring to here, in a systematic way all the way about.

From grades I would think one would be able to get an idea of what type of...were we dealing with longer fiber or shorter fiber that were produced.

I think those figures are available. They should even be available from commercial...I would think from, maybe from

A. (cont'd.) reports on production, commercial reports on production. But I stand to be corrected.

I know I do have some data from a number of years ago on the different grades that were produced in different years. But it's not complete for the whole industry, and I don't know whether it covers the period that one would be particularly interested in.

Q. Do I take it from what you have been saying that you don't have any judgement yourself as to whether what is being produced today is of a different dimension than what may have been produced in the mines ten years ago, twenty years ago?

A. Oh, I think that...as I might have mentioned earlier today...the...at the beginning of mining operations the emphasis was on long fiber production because it was...there were essentially manual methods for getting material out and hard labour in the mining operation to get it out...the emphasis on long fiber. Some of the short fiber, which was later of economic importance, would have gone out with the waste rock into the dump.

As time has progressed, shorter grades became of more importance. Now in this note, it addresses the question of the degree of openness of the fiber, whether or not the fibers... the degree of openness refers to, if you have a bundle of fibers, whether they get broken apart more. Obviously, that would be important in terms of whether or not more fibers of narrower diameter were being produced.

That part of the thing I can't answer, because the industry produce I don't know how many hundreds of different types of grade, but I am not sufficiently familiar with the differences between the four T's and the three S's, and the seven R's... and so on, to distinguish whether these reflect the different degrees of openness. But the industry certainly should

THE WITNESS: (cont'd.) be able to tell us what that means and whether the data exist to be able to use that type of information to see if there has been a change.

That would be my...

Q. Good..

A. But whether or not there have been surface area measurements which would tell us exactly whether this degree of openness exists, I don't know.

Next..we've seen that one.

Q. I don't mean to interrupt, but it might...maybe, Mr. Chairman, in view of the hour we should recess for about five minutes or ten minutes and have a cup of coffee and we might better see where we are going in terms...since it's Friday afternoon.

DR. DUPRE: Okay. Also consult...

MR. LASKIN: I'm going to consult the Commissioners as well as counsel.

DR. DUPRE: Okay. Shall we recess then for ten minutes?

MR. LASKIN: Sure.

THE INQUIRY RECESSED

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THE INQUIRY RESUMED

MR. LASKIN: During the recess, Dr. Gibbs has been kind enough to accommodate this Commission once again and has agreed to come back to a time convenient to him and ourselves, which probably won't be until October, so that we have suggested that he complete his remarks on this particular topic and then we adjourn for the day, and return when everybody will have a chance to question - if that's satisfactory.

DR. DUPRE: I can only say that this will make

DR. DUPRE: (cont'd.) us indebted to Dr. Gibbs three times over, having come in with a double debt this morning, and just from my conversation here with him and my fellow Commissioners, it would look more like very late October or perhaps early November, and quite likely it might be a Saturday.

MR. LASKIN: Q. Dr. Gibbs, I think we left off before the recess with what Mr. Hardy called the cliff-hanging question of 'what is a dangerous fiber'.

THE WITNESS: A. Can I have the...yes.

Q. Mr. Hardy found it so cliff-hanging that he failed to....

A. The question in terms of mesothelioma production that has come up on a number of occasions is whether or not a particular size of fiber, both diameter and length, is important in mesothelioma production. Stanton's early experiments and his latest series of experiments using glass fibers was directed at answering that question.

Now, what he found and what Wagner has suggested is that the fiber has to be fairly narrow - less than a quarter or a half a micron in diameter - and should be fairly long, and the original range of Stanton's diameters were fairly big, up to about two point five microns in diameter. More recent data suggested a diameter perhaps less than a quarter, half to a quarter of a micron, and with length perhaps greater than eight microns.

When we did this series of analyses, we had decided on a breakoff point of five microns and of point five microns. There are some subsequent data which are in the report by Mr. Wang, where for optical work we have looked at a limit of visibility of the optical microscope, and I think in our crocidolite paper we have taken point two microns and point three microns as that cutoff point.

THE WITNESS: (cont'd.) But we can get some idea of the differences between the different fiber types by taking a look at this diagram. If Stanton's data are correct for fibers less than point five micron in diameter, and length being quite long, you would expect that fibers less than half a micron and greater than five microns to perhaps be highest in the fiber type which might have the greatest potential to produce mesothelioma, if we used a similar dose for dose.

In fact what we see is that amosite tends to have, because of its predisposition to have long fibers, in fact has the highest proportion of fibers less than point five and with length greater than five microns, compared with chrysotile and crocidolite, with chrysotile having the lowest percentage.

Now, if we...while I've got this, and we'll come back to that in a moment...but while I've got this table on the screen, we can also look at some of the other differences.

If we look at the percentage of fibers greater than five microns in length, amosite shows up as number one, crocidolite considerably less, but greater than chrysotile.

If we look at the greater than ten microns in length, now thinking about length as very important, crocidolite and chrysotile now come closer together and bagging in amosite stands out again as having the very long fibers.

So on a length basis, if length is important to production, alone is important in production of mesotheliomas, amosite should be worse than the other types.

If we look...while I've got this on here it's useful to look at the percentage of fibers which might be seen on the optical microscope.

On line two, I've put the percentage of fibers greater than half a micron in true diameter, and greater than five microns in true length. What we have found from subsequent

THE WITNESS: (cont'd.) studies is that the limit of visibility of a fiber on the optical microscope is actually down around point two one microns, and we did that by making direct comparison between electron microscopic observation and optical microscopic observations on fibers on the same filter.

The reason we see them down as low as point two microns, point two one microns, is probably related to refraction and other factors which give us...though we are not seeing a clear image, but we are getting the impression that there is a fiber still visible at that limit. So for fiber observation it is possible to see fibers down to point two, point two one microns.

We have taken point five microns here and what you can see is that if we were counting amosite, we would see eight point one percent of fibers. If we were counting crocidolite, we would only see point two percent of fibers. With chrysotile we would see one point two percent of fibers.

So if we set the same standard for all fiber types, and assuming our microscope was tuned so we couldn't see fibers less than point five microns, we would end up with a considerably higher concentration of crocidolite in the atmosphere for the same concentration if we applied the standard optical microscope method of counting fibers, because we would readily see the amosite fibers and not readily see the crocidolite fibers.

DR. DUPRE: Could I just ask you, Dr. Gibbs, what the relationship, if any, is between these percentages and the data that you report in what is for our purposes your tab nine paper?

THE WITNESS: That's the scanning electron microscopic observation?

DR. DUPRE: That's it.

THE WITNESS: Yes. These measurements in this paper

THE WITNESS: (cont'd.) were carried out on samples collected in a totally different work environment, and also using a totally different...you know, using scanning electron microscopy...so a direct comparison between the two cannot be made.

DR. DUPRE: These data are not in another paper that has been in your exhibit, are they?

THE WITNESS: Yes. These figures are taken from the Lyon conference paper. That's exhibit...

MR. HARDY: It's number forty-five in your CV, but I don't think we have it in the collection.

THE WITNESS: Well, I do have a copy...

DR. DUPRE: Maybe you could just give it to counsel and we'll make sure..

MISS KAHN: We've got it.

DR. DUPRE: We do have it? Oh, okay.

THE WITNESS: So these are figures that are...these are figures which come not in...I don't think there is a table in that paper in this form, but these are figures that are derived from that.

Could I have the next slide, please?

The figures I showed you just now were for the bagging. Now if we look at the figures for the mining we get a very similar pattern. The numbers have changed a bit from the previous diagram, but again mining of amosite seems to have a higher proportion of fibers in this long, narrow category. It also has a higher proportion of long fibers altogether.

If we look at the very short, very narrow...very long fibers, the ones less than point five microns in true diameter, length greater than ten microns, we find that chrysotile contains a very small proportion of those overall, in comparison with the amphibole types.

Again, we have the same problem with the, potential

THE WITNESS: (cont'd.) problem for optical counting. If we wanted to consider the total airborne dust cloud as the important parameter, fibers greater than five microns and greater than point five microns would not equally represent the different fiber types.

Can I have the next, please?

What I have done, this information is not available at this point. It is taken from a report on some material, a study which I did some time ago, for a company in the U.S.A.

They had been interested in whether or not, what were the dimensions of their airborne fibers in their plant, and I was interested in the dimensions of fibers in textile operations.

We have complete freedom to publish these materials, but I have not published it at this point, so the only source of information is in the report to the company. I'm not quite sure I can make a copy of this table available to you. I'm not in a position to make the total report available to you because it was on the grounds that it would be to them, but with complete freedom to publish anything that came out of it.

But this table I think is of some interest because as we move into the carding operations and into the spinning, twisting, winding operations, beaming operations and so on, we have some idea from this about the proportion of narrow, long fibers that exist in those environments.

Now, the problem is that the data generation was separated by, I guess a couple of years from the time we did the mining measurements to the time we did these, and the person who did the mining measurements right the way through did not do all of these measurements.

The techniques that were applied in both series of measurements were the same, but some of the people were not

THE WITNESS: (cont'd.) the same, so there is that problem.

But you can see that in certain component parts of the process, the proportion of fibers greater than five microns and narrow, were quite large in contrast to what we saw in the mining industry.

I don't know whether there is any way you can flick this back and forward to the previous slide, but just so that you get an idea of what the situation...what the previous one showed.

Yes. If we look at the less than point five and greater than five microns in mining chrysotile, we are talking point eight percent here.

If I can go back to this one - we are talking...

DR. MUSTARD: Could you explain what S M P on that slide...?

THE WITNESS: Sorry. That's stationary samples or personal samples, and in this plant we took some measurements at fixed locations and some measurements on fibers in the breathing zone.

DR. MUSTARD: I understand.

And the D less than zero point zero four on the far right, is that...

THE WITNESS: That's the diameter...

DR. MUSTARD: Does that have any importance here?

THE WITNESS: No, what we were...yes, the reason for putting that in, we were interested in what proportion of the airborne fibers might be considered single fibrils. A single fibril of chrysotile would be somewhere between point zero one, perhaps, and maybe point zero four or point zero five.

Two hundred and fifty angstrom or point zero two five microns is considered to be a single fibril, normally.

THE WITNESS: (cont'd.) So that gives an idea of what proportion of single fibers were present around...

DR. MUSTARD: And it includes fibers of all lengths, I take it?

THE WITNESS: Of all lengths, yes.

So this gave us some indication that within the textile industry the proportion of fibers in that size range varied quite markedly from small loom in weaving, you can see we are down at point eight, which is comparable to the mining end, up to twenty-one percent of fibers in that size in the gang spinning area.

But on general, the proportion of longer fibers in that area seems to be substantially more than in the mining end.

Now, I know I have to caution against some of the limitations that we are faced with.

First of all, I mentioned that we had, in the mining industry they produce a whole variety of different grades of fiber. As we move into secondary industry, it is possible that for certain purposes in textiles, very long fiber might be a starting point for the process. If you go into certain pipe manufacture, shorter fiber might be the starting point for the process.

So we do not have at this point any indication of how the grade of fiber that we start with, the degree of openness of the fiber that might be introduced into a particular plant, affects this ratio. All we can do is report that at the time that we made the measurements in this plant, with the materials that they had, this is the proportion of materials .... the fibers in those airborne dust clouds.

So we cannot infer that fibers from the mine get broken apart to produce this. All we can say is that under

THE WITNESS: (cont'd.) normal operating conditions, as far as we knew them in this textile plant, this is the distribution of fiber dimensions we obtained, and in the mining industry under the normal operating conditions as we knew them, this is the distribution we got, and the two appear to be somewhat different.

Can I have the next slide, please?

Now, back to this million dollar question of what is the dangerous fiber. In Stanton's work he had suggested that fibers which were less than a quarter of a micron in diameter and with lengths greater than eight microns were...had a very high probability of producing mesotheliomas when implanted on the pleurae in his experimental animals.

What we did was to say, if we take our amosite, crocidolite and chrysotile data and we apply to the distributions by size that we have, go through the same calculations that Stanton went through, and see whether if we were to apply the distribution by size of the fibers which exist in the mining and bagging areas were to be implanted on the surface of the rat, would they...what probability of producing tumor would they have.

So we looked at the log of the number of particles, or number of fibers per microgram, in the same way that Stanton did, and we looked, and the values we got are shown at the top. For crocidolite mining - six point one one, bagging - six point two, amosite mining - five point eight nine and five point nine eight, chrysotile mining - six point seven five and six point two nine.

Now, if we now look at Stanton's probability of tumor production, we can see for each of the varieties of asbestos, for the airborne size distributions we measured, they all have greater than six or they have close to six, at least ninety percent and maybe close to a hundred percent probability of producing tumors, according to the Stanton model.

THE WITNESS: (cont'd.) Now, we know in practice from the epidemiological samples that in chrysotile miners we really don't have a hundred percent tumors, mesothelioma tumors in our chrysotile workers. So either one or two things are...there are a number of factors which must be very important in addition to these fiber sizes, in the production of the tumor. From a straight look in terms of Stanton's model, that's what we get - that we cannot distinguish the fiber types in terms of their production, possibility of producing tumors.

What we had hoped would happen out of this, which would have been rather nice, was that one of the fiber types would have fitted nicely on here with a high probability, and the other fiber type with a low probability, and we could have then gone back to an animal experiment somewhere to verify that.

But it looks as if each of the fiber types from the size distributions we have don't distinguish in that way.

So I think that...can I just see...yes, all right.

There was just one other comment I might make on the dimensional question, and this of course goes into the fiber counting methods in the future.

There has been a lot of argument over the years whether three to one, five to one or ten to one is an appropriate ratio to use for fiber counting. In fact, we've looked at this question in crocidolite. It is not yet finalized in looking at it in terms of chrysotile and amosite. I suspect amosite will be very similar.

Whether or not if you applied a ten to one ratio for optical counting, in which case you can readily distinguish a fiber, would you get, would you be losing much in the way of counts?

Now, using the electron microscope we have looked

THE WITNESS: (cont'd.) at the effect of size and the aspect ratio in terms of what we would count, and we found that in general if you used a five to one or a ten to one to count fibers using optical microscopy, you really would not lose too much. The counts would not be that much different from what you get when you count three to one, and at the same time you would find it would be easier to define a fiber, to be sure that you are counting a fiber.

On the other hand, if you go to electron microscopy, now you have so many fibers which are very, very short and not very long, you have a very high proportion of fibers which are of diameter less than point zero five microns and with length less than point two microns or thereabouts, a very high proportion of fibers up in that size range, and as a result if you were to switch to a five to one or a ten to one ratio you would now perhaps decrease to a fairly large extent your count of electron microscopic fibers, and there is no magic right or wrong, but I'm just pointing out that for optical counting it wouldn't matter too much in terms of increasing the ratio, but for electron microscopy one would miss particles which could be elongated and very small ones, if one were to change that parameter.

That comes out of the same observations.

MR. LASKIN: Q. I suppose I have a number of questions, Dr. Gibbs, just relating to your discussion, the talk that you have just given to us on mesothelioma and fiber density, and perhaps I'll reserve them until the time you come back, except for the one question which arises almost out of the last comment you made.

Given what your own research and your own research as applied to Stanton's hypothesis, do you yourself have any explanation that might reconcile that research with the epidemiological data as you perceive it?

THE WITNESS: A. Yes. I think that there is pretty good evidence to suggest that not only fiber dimensions are important in the production of mesothelioma. There is a question of durability that has been raised, and I think even Stanton raises it in his studies.

Q. What do you mean by that, just before you pass on?

A. That means the ability of the fiber to survive for some period of time, if you like, in the tissues prior to either dissolving or breakup or disappear. There is some evidence in the literature that this is an important factor, for example in fibrosis. There is a paper by Kuschner, Wright, or Wright and Kuschner, where they used glass fibers and some asbestos fibers, both short, and short, I think, was less than five microns, I'm not quite sure about their final definition of short, and long - which were fairly long fibers, greater than twenty microns or so. They put these fibers into rats or guinea pigs. They produced fibrosis with glass fibers, but not to the same degree as they produced fibrosis with asbestos...but only with the long glass fiber.

When they looked in the lung, they found there were many more short glass fibers in the lung than there were when they started, so they concluded that probably the glass fiber broke up, and this would be an example of durability in the sense that the fiber has to stay around in its original state for some period of time to initiate a reaction or to cause a reaction.

Q. Can you apply that principle specifically to, for example, chrysotile? What do you suspect might be happening?

A. Well, the chrysotile story is not quite clear. There have been...if one were to believe the Stanton model that

A. (cont'd.) you have to have long, there is a much higher probability of producing mesotheliomas with long, narrow fibers than short fibers, then you would expect for very long fibers and narrow fibers to have to get to the pleurae in order to produce those tumors.

Now, there have been a few reports - among them Sebastien, for example - describing the sizes of fibers observed on the surface of the pleurae.

Now he has observed fibers on the pleurae, but as I recall them, they were relatively short fibers on the pleurae, and not long fibers.

Now, the other problem I guess, in interpretation of the Sebastien data, is that his people are chrysotile-exposed people and therefore if you expose somebody to chrysotile, you would expect to find chrysotile on the pleurae and not other fiber types, generally.

The information we have from observations of where fibers get to in the human is very, very limited. The fact that he finds only short fibers on the pleurae, and some people would infer the short fibers are responsible for the mesothelioma, to me might suggest that perhaps there is a pretty good filter mechanism which either prevents long fibers getting to the pleurae, in general, some breakdown must occur or some...for long fibers to get to those areas. Or the probability of long fibers getting to those areas is very, very slight.

We see the distributions here of long fibers of chrysotile - there was a fairly, slightly lower proportion of long, narrow chrysotile fibers than crocidolite.

Now, if you were to compare the probability of those fibers reaching the pleurae, then you would expect for a higher probability for the fibers which contain the higher proportion of fibers in that size range.

A. (cont'd.) Hence, chrysotile under that circumstance would lead a little bit in terms of its potential to get fibers there to start with.

Whether or not solubility of fibers plays a role, whether or not dispersion of fibers once they are deposited plays a role, I think there is evidence available on that, but I don't think that it's yet sufficiently concrete as to be able to say yes, indeed, that is the reason why chrysotile does not appear to produce mesotheliomas in man as frequently.

Because in animals, chrysotile does produce it as frequently, when you put the fibers right there.

Q. I'm not certain that I understand what your ultimate view or judgement on this particular issue is. On durability, do you yourself see a distinction between...or among the fibers types, and if so, what do you suggest accounts for it?

A. I think if one looks at the chemistry of the amphibole fibers and chrysotile, the amphibole fibers are very acid-resistant, they are certainly not water soluble. Chrysotile, on the other hand, has been demonstrated not to be very acid-resistant, and it has a magnesium hydroxide outer layer so the possibility for some leaching to occur does exist.

There is another difference between the amphiboles and chrysotile. That is that the ability to break apart, of a bundle, is far greater for chrysotile than it is for the other fibers. The other fibers tend not to occur as nice bundles where the fibers will keep peeling off, whereas chrysotile does that.

It's possible, and it's only speculation on my part, that maybe when the very narrow fibrils are broken off that they may tend to, in solution, to become little balls, if you like. Then they would behave and be dealt with very much like other particles, with macrophages.

This ability for fibers to break apart might

A. (cont'd.) improve their ability to get removed from the lung.

I have no evidence to show that that's so. I don't know if anybody else has any evidence to show that that's so, but the arguments that have been made by others in some cases have said, maybe these fibers get dispersed that way and then get translocated to other parts of the body...they just disappear from the lung and they go as single fibrils to all sorts of other locations.

But there is no doubt...well, I won't say there is no doubt, but it appears that we do not find as much chrysotile in the lung as one would expect to have with some of the exposures that occur.

Q. Any other general factor, apart from durability, that you might suggest?

A. In terms of etiological factors in production of mesothelioma? I'm sure there are many other factors that are important in terms of mesothelioma production, but I think size is an important one from what we know about experimental work, and durability is an important one. I think it is possible that host factors may play a role.

What I mean by host factors is individual differences might play a role. Again, I have no supporting - just saying if we are looking for other possible factors to consider, that might be one.

It appears that smoking doesn't play a role in this instance, and I don't know of any evidence that any other attached particles or whatever are important in that production.

MR. LASKIN: Well, perhaps we'll carry on, Dr. Gibbs, the next time we might.

We've all had a long day, especially you, so thank you very much and I suppose, Mr. Chairman, we adjourn

MR. LASKIN: (cont'd.) Dr. Gibbs' testimony until a time to be arranged, and we otherwise, I think, meet next Wednesday at two o'clock.

DR. DUPRE: Yes. We do meet next Wednesday at two o'clock, and Dr. Gibbs, your testimony simply stands adjourned sine die, with our utmost gratitude.

Thank you, indeed, Dr. Gibbs.

THE INQUIRY ADJOURNED

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THE FOREGOING WAS PREPARED
FROM THE TAPED RECORDINGS
OF THE INQUIRY PROCEEDINGS

Edwina Macht
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